

PHILOSOPHICAL
TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCLIII.

VOL. 143.—PART II.

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1855

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PHILOSOPHICAL TRANSACTIONS.

VII. *On the Eclipses of AGATHOCLES, THALES, and XERXES.*

By G. B. AIRY, Esq., Astronomer Royal.

Received December 15, 1852,—Read February 3, 1853.

SECTION I. *Preliminary.—On the Elements of the Lunar Tables.*

1. **TILL** the beginning of the present century, neither the mechanical theory of the moon's motion, nor the numerical determination of her principal elements, nor the lunar tables founded on these, were sufficiently accurate for the computation of a distant eclipse. And (perhaps in consequence of the evident imperfection of these essential grounds of calculation) the mode of treating chronological eclipses was, in most instances, extremely lax. The general result of these deficiencies is, that in any point of the slightest delicacy, the calculations made before 1810 are absolutely worthless.

2. The extension and general improvement of the lunar theory by LAPLACE, and in particular the determination of the secular equations depending on the square of the time, very greatly altered the state of lunar and chronological science. Partly by the stimulation of foreign academies, partly by individual enterprise, lunar tables were soon produced which embodied the principal results of the new theory, and which were founded on more numerous and more carefully reduced observations than had been used before. The extensive tables by BÜRO, printed by the Bureau des Longitudes in 1806, and the smaller tables by OLTMANN'S from the same elements, printed in the fourth supplementary volume of the Berliner Jahrbuch in 1808, will long be remarked as important steps in lunar calculation.

3. The first valuable deduction which was drawn from these, in reference to chronological computation, was the series of calculations in the paper by our late Fellow, MR. FRANCIS BAILY, "On the Solar Eclipse which is said to have been predicted by THALES," communicated to the Royal Society on 1811, March 14, and printed in the Philosophical Transactions for 1811. Although there can now be no doubt that the eclipse on which MR. BAILY fixed was a wrong one, yet this paper (the first, I believe,

of Mr. BAILY's astronomical publications) must be considered as possessing the highest value; for its due appreciation of the characteristic circumstances of a total eclipse, for its accuracy of computation, and for the caution and good faith with which the results are announced. Mr. BAILY, in the first place, pointed out that only a total eclipse could satisfy the account of HERODOTUS, and that a total eclipse *would* suffice. He lived to witness the total eclipse of 1842, but he observed it from a room of a house, where probably he could scarcely remark the general effect of the eclipse. I have myself seen two total eclipses (those of 1842 and of 1851), being on both occasions in the open country; and I can fully testify to the sudden and awful effect of a total eclipse. I have seen many large partial eclipses, and one annular eclipse concealed by clouds; and I believe that a body of men, intent on military movements, would scarcely have remarked on these occasions anything unusual. Mr. BAILY then, adopting BÜRC's tables, exhibited in detail the results of computation of the eclipses of B.C. 585, May 28; 583, October 1; 607, July 30; 597, July 9; 601, September 20; 603, May 18; 626, February 3; and 610, September 30; and stated that he had computed all the eclipses which seemed likely to have been visible in Asia Minor from B.C. 650 to B.C. 580. He found that only the eclipse of 610, September 30, passed over Asia Minor; that the centre of its shadow crossed the river Halys at its mouth, passing a short distance south of the Caspian Sea; and he adopted it as the true eclipse of THALES. But he then subjoined a computation which threw doubt on the whole. Upon applying the elements of the same tables to the computation of the eclipse of AGATHOCLES, B.C. 310, August 15, he found that they would not give a total eclipse for any place in which it was possible to locate AGATHOCLES at the time when, according to the historical record, the shadow passed over him. Mr. BAILY inferred from this that the tabular motion of the moon's node must be altered; and he admitted, as a probable effect of such an alteration, that the eclipse of B.C. 610 might be found insufficient for the history of HERODOTUS. But he confidently believed that no other eclipse between B.C. 650 and 580 would be made to pass centrally over any part of Asia Minor.

4. Since I first read this paper, I have always attached greater importance to the last portion of it than to any other part. It has always appeared to me that not the eclipse of THALES but that of AGATHOCLES ought to be considered as the true cardinal eclipse for chronology and lunar astronomy. And I have long since contemplated the recalculation of the latter eclipse with the view of obtaining correct elements for the computation of the former.

5. About the same time in which Mr. BAILY was employed in his calculations, Mr. OLTMANN was also engaged (apparently without any knowledge of Mr. BAILY's labours) on the same subject. *His paper was presented to the Berlin Academy on 1812, November 26, and is printed in the Berlin Memoirs 1812-1813. He points out (as Mr. BAILY had done) that a total eclipse is required: he insists also that it must have been visible as a large eclipse in Ionia; he then, using his own tables

(equivalent to BÜRG's), exhibits the detailed elements of several eclipses, and finds that the eclipse of B.C. 610, September 30, was total on the Halys and at Erzeroum (a result agreeing precisely with Mr. BAILY's), and, like Mr. BAILY, he adopts that as the true eclipse of THALES. He then subjoins a very valuable table, exhibiting roughly the track of the central shadow in every eclipse, whether total or not (113 in number), as computed by means of the same tables, from B.C. 631 to 585.

6. The successive alterations in the adopted motions of the moon's node have been nearly as follows. I must premise that there is much confusion among the original writers, from the circumstance that some in speaking of secular motions refer that term to a Julian century, while others use an ordinary Gregorian century, which differs from the Julian by one day, and produces a difference of $3' 16''\cdot 4$ in the motion of the node. I have endeavoured to remove this obscurity by giving the motion in all cases for a Julian century.

LALANDE, in the third edition of his Tables, 1792, had made the secular regression of the moon's node	134° 11' 15"
LAPLACE, in the Additions to the <i>Connaissance des Temps</i> , AN. VIII., from ALBATENIUS' discussion of PROTEMY, found that the motion was to be diminished to	134 8 25
BÜRG and OLTMANNS, 1806 and 1808, adopted	134 11 42
BOUARD, <i>Monatliche Correspondenz</i> , 1811, May, found from the discussion of numerous eclipses	134 9 42
BURCKHARDT's Tables, 1812, adopt	134 10 12
OLTMANNS, <i>Berliner Jahrbuch</i> , 1817 (printed about three years earlier), found from eclipses in 1239 and 1241	134 10 28
WURM, <i>Zeitschrift für Astronomie</i> , 1817, January and February, found from twelve ancient lunar eclipses	134 8 23·8
and from eight total or annular solar eclipses	134 8 25·6
DAMOISEAU's Tables, 1824, adopt	134 9 57·5

7. In the *Berliner Jahrbuch*, 1824, is a paper by OLTMANNS, dated 1821, May 15, in which for the first time Mr. BAILY's researches are mentioned. M. OLTMANNS remarks that the close agreement between Mr. BAILY's results and his own on the eclipse of THALES proves the correctness of their calculations; and then he proceeds to say that the researches of BOUARD, BURCKHARDT, and WURM, as well as his own, show that the secular regression of the node must be diminished $2'$ (or reduced to $134^{\circ} 9' 42''$); that with this the eclipse of AGATHOCLES was possible, supposing AGATHOCLES near Cape Passaro; that to make the eclipse central there the regression must be further diminished (as I understand him) by $18''$, and to make it barely possible it must be increased by $9''$. But after insisting on the certainty of this correction of the node, and after having called attention to his former calculations on the eclipse of THALES, he never so much as hints that his former conclusions must now necessarily be erroneous. I am wholly unable to account for this extraordinary silence.

8. In the *Ergänzungs-Heft* to the *Astronomische Nachrichten*, published in 1849, Professor HANSTEN has given a most interesting discussion of the total eclipse which occurred at the battle of Stiklastad, A.D. 1030, August 31. The certainty as to the exact spot on which this battle was fought, and the attainable precision of the astronomical determination (for the eclipse was annular when it commenced upon the earth, and the dark shadow was therefore extremely small at Stiklastad), will render this eclipse unusually valuable. Professor HANSTEN infers from it the value of the secular regression of the node $134^{\circ} 8' 28''.5$; but as it appears that in the calculation (which was avowedly made rather for the verification of history and chronology than for the correction of the lunar elements) BURCKHARDT's tables were used without alteration, and as we shall hereafter give reason for supposing that, in one important point, these tables are incorrect, this result is not entirely free from suspicion. It is much to be wished that Professor HANSTEN would calculate this eclipse with special reference to the correction of the elements of the moon's motion.

9. In the year 1846, the reduction of the Greenwich Lunar Observations from 1750 to 1830 was nearly completed under my direction, and I was able to exhibit the apparent errors of the moon's epoch of mean longitude to Professor HANSEN. The immediate result of this was, that Professor HANSEN discovered two inequalities of long period in the moon's motion, produced by the attraction of Venus. Their formulæ are given in the *Astronomische Nachrichten*, No. 597. The values of the coefficients, as I understand, are not entirely free from doubt.

10. In a paper communicated to the Royal Astronomical Society on 1848, June 9, and printed in vol. xvii. of their *Memoirs*, I gave the principal results of the Lunar Reductions above cited. It is unnecessary to recapitulate here the corrections to the various elements of the moon's orbit for different years; I shall only remark, that though I do not doubt that the mean motion of the node may be found from them far more accurately than it was ever before found from meridional observations, yet I conceive it to be still open to correction from observations of distant eclipses. Indeed, the uncertainty, as to the part of the field of view at which the Greenwich observers in BRADLEY's time were accustomed to observe the moon's declination, leaves considerable uncertainty as to the place of the node.

11. In alluding to these corrections, it is proper to advert to the changes which their apparent values must receive from HANSEN's new inequalities. And first, as to the change in the correction to the moon's motion of mean longitude. HANSEN's inequalities affect the apparent error of epoch very differently in the first part and in the last part of the interval through which the reductions extend, the numerical amount of their influence going through a gradual change without repeated reversion of sign. For the time, therefore, through which the reductions extend, these inequalities produce an apparent alteration in the moon's mean motion; and therefore, when, in the comparison of observations with theory, they are duly taken into account, the resulting value of the moon's secular motion in longitude will be sensibly different

from that which would have been found if they had not been considered. This change is fully recognized in the paper in volume xvii. of the Royal Astronomical Society's Memoirs.

12. Secondly, as to the correction to the motion of the moon's node. The comparison, between an observed latitude of the moon (supposed near the node) and the corresponding tabular latitude of the moon as calculated from DAMOISEAU's tables, gives in fact a comparison between the true argument of latitude and DAMOISEAU's argument of latitude; and therefore it gives the sum of two corrections to two different elements of DAMOISEAU's tables; namely, the correction to DAMOISEAU's longitude of the moon, and the correction to DAMOISEAU's supplement of longitude of moon's node. And when, from the groups of results, we infer the secular correction to the motion of the argument of latitude, we have found the sum of two corrections; first, the secular correction to DAMOISEAU's mean motion of the moon, such as will best reconcile DAMOISEAU's longitude of the moon, unaffected by new inequalities, with the observed longitude of the moon, during the period to which the reductions apply; and, secondly, the secular correction to DAMOISEAU's regression of the node.

13. Now for the difference between DAMOISEAU's longitude unaffected by new inequalities, and the observed longitude, we must refer to the second column of the table, Royal Astronomical Society's Memoirs, vol. xvii. p. 35; and, comparing the first four numbers with the last four, we find the correction to DAMOISEAU's motion of the moon during 38.1 years = $-0''.30$. But in page 54, the correction to DAMOISEAU's motion of argument of latitude during the same time is $-24''.26$. Hence the correction to DAMOISEAU's regression of the node during that time is $-24''.26 + 0''.30 = -23''.96$, or the secular correction is $-62''.9$. Applying this to DAMOISEAU's secular regression in a Julian century (namely, $134^\circ 9' 57''.5$), we find the corrected regression $134^\circ 8' 54''.6$, which is probably the most accurate that can yet be deduced from meridional observations.

14. But, in correcting DAMOISEAU's tables for further use, we must not apply the quantity $-62''.9$ to his secular motion of argument of latitude. The moon's tabular longitude at any time, putting H for the value of HANSEN's inequalities at that time, will require (see page 36 of the same Memoir) the correction $+39''.3 \times$ number of centuries $+H$; and as the tabular supplement of node requires the correction $-62''.9 \times$ number of centuries, the tabular argument of latitude (which is the sum of moon's longitude and supplement of node) will require a correction equal to the sum of these two corrections, or $-23''.6 \times$ number of centuries $+H$. For a distant eclipse, the quantity H may be safely neglected.

15. Thirdly, as to the correction to the motion of the moon's perigee. The same considerations, in every respect, which have been used for determining the correction to the motion of node and to the motion of argument of latitude, are to be used for determining the correction to the motion of perigee and to the motion of mean anomaly. Thus, in p. 40 of the Memoir above cited, the apparent correction of motion

of anomaly in 38¹ years is $-12''\cdot03$: by the second column of the table on p. 35, the apparent correction of mean motion in the same time (the tabular values being unaffected by HANSEN's inequalities) is $-0''\cdot30$; hence the true correction to the motion of perigee (estimated as a regression) in 38¹ years is $-12''\cdot03 + 0''\cdot30 = -11''\cdot73$. And the true correction to the moon's motion of mean longitude in 38¹ years is $+14''\cdot99$. Therefore the true correction to the motion of the mean anomaly in 38¹ years is $+14''\cdot99 - 11''\cdot73 = +3''\cdot26$; and the true secular correction is $+8''\cdot56$. This supposes that HANSEN's inequalities in longitude are not accompanied with sensible inequalities in the place of perigee.

16. The position of the moon at any time, as affecting the circumstances of an eclipse, will depend on the moon's mean longitude, the longitude of perigee, and the longitude of node. The values of these three elements for any late year are known with very great accuracy (their values for certain years are given in the Memoir repeatedly cited); and the annual motions of mean longitude and longitude of perigee for a Julian century at the present time are very accurately known; in that of the longitude of node there is a very minute uncertainty. But the secular motion of each of these elements changes from century to century; and terms are thus introduced into the expression for each of these elements depending on the square and higher powers of the time. LAPLACE was the first who computed (from theory) the coefficients of these terms; and his numbers were adopted, with insignificant alterations, in BÜRG's and BURCKHARDT's tables. DAMOISEAU, on repeating the investigation, obtained different values for the coefficients; in particular, he introduced in the coefficient which relates to the place of perigee a change of such magnitude as very greatly to modify the circumstances of any calculated distant eclipse. PLANA and HANSEN, by independent investigations conducted in different ways, have in general confirmed these alterations; the result, however, of HANSEN's last investigation differs somewhat from that of his former investigation, though by a very much smaller quantity than the difference of each from LAPLACE's values. I shall give here the coefficients of the square of the number of centuries obtained by these writers; the signs of those numbers which relate to perigee and node being applicable to progression of perigee and regression of node. The reader must remark that a change of $1''$ in the coefficient for mean longitude, of $9''$ in that for longitude of perigee, or of $11''$ in that for longitude of node, produces, in the moon's place for a perigee eclipse at the time of THALES, an effect of about $10'$, and that this will alter the place of the eclipse-shadow at a given time not less than 10° on the earth's surface.

LAPLACE, *Mécanique Céleste*, vol. iii. pages 237, 273, 274.

Coefficient for mean longitude	$+10''\cdot18$
Coefficient for longitude of perigee	$-30''\cdot55$
Coefficient for suppl. longitude of node	$-7''\cdot49$

BÜRG'S and BURCKHARDT'S Tables.

Coefficient for mean longitude	+10 ⁰⁰
Coefficient for longitude of perigee	-29 ⁹⁸
Coefficient for suppl. longitude of node	- 7 ³⁵

DAMOISEAU, *Mém. Savans Etrangers*, Third Series, tome i. p. 544, and Tables.

Coefficient for mean longitude	+10 ⁷²
Coefficient for longitude of perigee	-39 ⁷⁰
Coefficient for suppl. longitude of node	- 6 ⁵⁶

PLANA, *Théorie de la Lune*, vol. i. p. 724.

Coefficient for mean longitude	+10 ⁵⁸
Coefficient for longitude of perigee	-40 ²³
Coefficient for suppl. longitude of node	- 6 ⁷⁹

HANSEN, *Astronomische Nachrichten*, No. 443.

Coefficient for mean longitude	+11 ⁹³
Coefficient for longitude of perigee	-39 ¹⁸
Coefficient for suppl. longitude of node	- 6 ⁴⁹

HANSEN, *Astronomische Nachrichten*, No. 597.

Coefficient for mean longitude	+11 ⁴⁷
Coefficient for longitude of perigee	-36 ³¹
(The coefficient for suppl. longitude of node is not computed).	

17. I believe that I have now stated, without important omission, the progress of Lunar Theory, as bearing on distant eclipses, to the present time; and I shall now proceed with the special calculations of this paper.

SECTION II. *Methods of Computation adopted in this Paper.*

18. The tables used in these computations, for the sun's longitude and the obliquity of the ecliptic, are CARLINI's tables attached to the *Effemeridi di Milano*, 1833. The precepts of the tables have been strictly followed, with these exceptions; that from Table V. the number has been taken which corresponds to lunar syzygies, and the mean values of Table VI. and Table XIII. have been taken, and the sum of these numbers or 7⁵ has been used as a constant. The sun's semidiameter has been slightly altered for the change of excentricity of the earth's orbit. The sun's longitude is found for two adjacent hours, and is then changed into right ascension and north polar distance. The following error has been remarked in the printed tables: Table XXX., for 16' 44¹⁰ read 16' 34¹⁰.

19. The lunar tables employed here are the same (except in the epochs) as those used in the Reduction of the Greenwich Lunar Observations. With the exception



of some small inequalities and some small changes of coefficients, entirely insignificant in the computation of an ancient eclipse, they are the same as DAMOISEAU's tables.

20. The arguments which DAMOISEAU distinguishes by the letters u , x , t , z , y , are formed in the following manner. To DAMOISEAU's epochs, for the year in the nineteenth century which differs by a whole number of centuries from the year for which the calculation is to be made, are added the numbers in his Table II. for the whole number of centuries taken backwards, and the corrections for u and z given at the foot of that table, and the corrections proportional to the square of the time in Table III., and the motions for $12^h 9^m 21^s.5$ (to reduce Paris civil time to Greenwich astronomical time), and the numbers for the month, day, and hour of Greenwich mean solar time. The numbers thus formed are called DAMOISEAU's Elements. It will be seen from this statement that I have adopted DAMOISEAU's coefficients of the terms depending on the square of the time. Then the following corrections are added. For u (the mean longitude), the secular motion is increased by $+1' 21''.4$, reckoning the years from 1814; the same correction is applied to t (the moon's elongation from the sun). For x (the mean anomaly), the secular motion is increased by $+26''.4$, reckoned from 1788. For y (the argument of latitude), the secular motion is increased by $-72''.8$, reckoned from 1782. These are called, in the subsequent articles, the Greenwich Corrections. By the addition of these, values of u , x , t , z , y are formed which are called the Unvaried Greenwich Elements, and these are the fundamental arguments used for the calculation of the moon's places. The longitude and ecliptic polar distance thus found for a certain hour are altered by the horary motions found from the tables, and longitude and ecliptic polar distance are thus obtained for a second hour. These are converted into right ascension and north polar distance with the obliquity found in the solar calculations. The following error has been remarked in DAMOISEAU's tables: Table II., —2200 years, for $113^c 6634$ read $113^c 8634$.

21. With these right ascensions and declinations of the sun and moon, the circumstances of the eclipse have been computed by the use of WOOLHOUSE's methods in the Appendix to the Nautical Almanac, 1836.

22. The next step is, to examine the effect of a possible change of elements. And here it may be remarked, that when the track of an eclipse is not highly inclined to the parallel upon the earth (which is true with regard to the eclipses here under consideration), a small change in the moon's longitude produces little effect in the track of the eclipse. Partly for this reason, and partly because the place of node appears liable to the greatest uncertainty, I have recognized no error in the moon's place except as depending on a possible error in the argument of latitude. In order to take account of this, I have in each calculation increased the argument of latitude by $20'$ centesimal; and this changed element, taken in combination with the other elements unchanged, constitute the system which I call "Elements with Variation." With this new system of elements, the moon's place is computed and the track of the central part of the shadow is computed, exactly as with the "Unvaried Greenwich Elements." The breadth of the shadow, as laid down on an ordinary chart, is

assumed to be the same as with the "Unvaried Greenwich Elements." The further inferences, as to the multiple, positive or negative, of "variation" (always using that word in the technical and precise sense of "increase of argument of latitude by 20'") which is best adapted to the circumstances of the problem, are deduced by graphical process, as will be seen hereafter.

23. The whole of the calculations have been made by Mr. H. BREEN, Assistant at the Royal Observatory. I have every reason to trust in their accuracy.

SECTION III. *Eclipse of AGATHOCLES.*

24. The account given by DIODORUS, lib. xx., and supported in all important particulars by that of JUSTIN, lib. xxii., is as follows. AGATHOCLES, being blockaded in Syracuse by the Carthaginian fleet, secretly formed the design of invading the Carthaginian territories, and placed men on board ships in the harbour, but was unable for several days to pass the enemy's fleet. At length a convoy of provision-ships appeared; the blockading ships left their station to attack the convoy; AGATHOCLES took the opportunity of leaving the harbour; the Carthaginians immediately left the convoy and followed him; he escaped with difficulty under cover of night; and "the next day there was such an eclipse of the sun that the day wholly put on the appearance of night, stars being seen everywhere." After he had sailed "six days and the same number of nights" he made the African shore, and again barely escaped a Carthaginian fleet (it does not appear whether it was the same as that which had blockaded Syracuse; it was probably a different fleet); and landed at a place called "The Quarries." He shortly took two cities, of which the second (White Tunis) was 2000 stadia from Carthage; it does not appear however whether the distance from Carthage was measured in the most direct line or in reference to the route afterwards pursued by AGATHOCLES; and there is no mention of the distance or direction of the city from the landing-place. It is stated by DIODORUS that the troops, before sailing, supposed that they were to make an attack either on Italy or on the Carthaginian part of Sicily; and by JUSTIN, that, while on the voyage, they supposed that they were going on a marauding expedition either to Italy or to Sardinia.

25. The eclipse was evidently total; and the principal task which remains for us, in order to render this eclipse available for the correction of the lunar tables, is, to investigate from these materials the probable place of AGATHOCLES when the shadow passed over him. The first thing is, to discover the position of his landing-place. Mr. BAILY supposed this to be in the Gulf of Khabes. By the assistance chiefly of Captain WILLIAM HENRY SMYTH, R.N., I am enabled to indicate, and (as I conceive) with perfect certainty, a very different locality. On the west of Cape Bon, at a place called Alhowareah, are quarries of immense extent, proceeding from the sea cliffs and worked into the solid rock, and lighted by holes from above. They are undoubtedly the quarries from which Utica and Carthage were built. "Alhowareah" appears to be a corruption of the Roman name "Aquilaria;" the place at which CURIO

landed *b.c.* 49. It would appear to have been a usual place of landing, at least in coming from Sicily. It is said to be a well-sheltered harbour (which indeed is implied by its use as a shipping port for such large quantities of stone); and the great height of Cape Bon renders it an admirable point to approach from the sea. There can be no doubt that Alhowareah is the place where AGATHOCLES landed. In some maps there is marked in this district a valley called "Wady Abiad," "the white valley;" it is possible that White Tunis may have been situate here.

26. The adoption of Alhowareah as the landing-place of AGATHOCLES leaves the question open whether AGATHOCLES sailed on the north side of Sicily or on the south side. I am entirely indebted to J. W. BOSANQUET, Esq. for the suggestion that AGATHOCLES may possibly have passed the straits of Messina and sailed on the north side of Sicily; and I am also indebted to that gentleman for the heads of the following reasons for supposing that AGATHOCLES really did take the northern course.

(A.) The distance from Syracuse to Alhowareah by the southern route is about 200 nautical miles; that by the northern route about 330 miles. Either of these distances is considerably less than we should expect a fleet to traverse in six days and nights (according to the usual rate of sailing of ancient ships); and, so far, the northern route, as being the longer, is the more probable of the two.

(B.) Selinus, Himera, and other towns in the extreme west of Sicily, had always been Carthaginian. Agrigentum had been maintained in the Carthaginian interest even before the battle of Himera. After that battle, all the Greek cities on the northern coast, and all north of Syracuse on the eastern coast, and even Camarina on the south, submitted to the Carthaginians. Gela alone was firm in the interest of Syracuse. The predominant party there was supported by a Syracusan garrison; and the town was so strongly fortified as to defy the attack of HAMILCAR. The expression "campi Geloi" of VIRGIL, contrasted as it is with "Acragas magnanimū generator equorum," seems to imply a great breadth of corn-lands; and we know from DIODORUS that the harvest was just gathered in. It cannot, I think, be doubted that the provision-ships, whose approach drew off the attention of the blockading ships, were coming from Gela; in that case, they approached from the south; the blockading ships therefore started towards the south to attack them; and AGATHOCLES, as soon as he passed out from the mouth of the harbour, necessarily went towards the north.

(C.) The belief of the troops that they were on their way to Italy or Sardinia implies that they were on the northern route.

27. I have no doubt that AGATHOCLES did really take the northern course. But as the usual opinion is that he took the southern course, I think it proper to exhibit the results of calculations made on both suppositions. As we do not know the hour of day at which AGATHOCLES sailed out of harbour, and as we have no information on the comparative rate of sailing on the different days, we cannot judge very precisely on the place of AGATHOCLES at the time of the eclipse ($7\frac{1}{2}$ A.M. on the next morning). But it seems likely that the following may be near enough to the truth:

Possible northern position of AGATHOCLES } Latitude $37^{\circ} 55'$. Longitude E. $15^{\circ} 30'$.
 at the time of the total eclipse

Possible southern position Latitude $36^{\circ} 35'$. Longitude E. $15^{\circ} 0'$.

28. The calculations of the places of the sun and moon are made in the manner described above, for the times -309, August 14, 19^h and 20^h, Greenwich Mean Solar Time (or, in civil reckoning, B.C. 310, August 15, 7^h and 8^h A.M.). The following are the fundamental arguments of the moon's place for -309, August 14, 20^h, G.M.S.T.

	<i>a.</i>	<i>z.</i>	<i>d.</i>	<i>z.</i>	<i>y.</i>
DAMOISEAU'S Elements	149-64068	26-1319	395-6437	283-4513	1-6953
Greenwich Corrections	-25737	-0554	-2574		+ 1522
Greenwich Unvaried Elements.....	149-38331	26-0765	395-3863	283-4513	1-8475
Elements with Variation	149-38331	26-0765	395-3863	283-4513	2-0475

With the Greenwich Unvaried Elements, the following places are obtained :—

	19 ^h .	20 ^h .
Obliquity of Ecliptic.....	23° 43' 42-0	
Sun's Longitude.....	136° 33' 57-6	136° 36' 24-1
Sun's Right Ascension	139° 4' 59-5	139° 7' 24-7
Sun's North Declination	16° 3' 39-0	16° 2' 54-4
Sun's Semidiameter	15 56-7	
Moon's Longitude.....	136° 1' 19-8	136° 38' 52-6
Moon's North Latitude.....	16 17-3	19 45-6
Moon's Right Ascension	138° 37' 44-4	139° 16' 8-1
Moon's North Declination	16° 29' 3-3	16° 20' 58-4
Moon's Equatorial Horizontal Parallax	61 1-4	
Moon's Geocentric Semidiameter.....	16 40-1	
Sun's True Right Ascension, in time		9 ^h 16 ^m 29 ^s 65
Sun's Mean Right Ascension, in time.....		9 ^h 14 ^m 10 ^s 76

From these, the following numbers are deduced :—

Greenwich Mean Solar Time of conjunction in R.A. —309, Aug. 14^d 19^h 45^m 27^s.

R.A. of Sun and Moon at conjunction $139^{\circ} 6' 49''\cdot 5$.

Sun's North Declination at conjunction $16^{\circ} 3' 5''\cdot 2$.

Moon's North Declination at conjunction $16^{\circ} 22' 56''\cdot 1$.

Greenwich Mean Solar Time of Middle of General Eclipse 19^h 52^m 28^s.

Using $\frac{1}{300}$ for the earth's compression, the following coordinates of points on the earth's surface are obtained; which it must be remembered are deduced from Greenwich Unvaried Elements.

Points on the central path of the shadow.

Greenwich Mean Solar Time.	East Longitude.	North Latitude.
h m s	° ' "	° ' "
18 20 0	7 2	34 37
18 22 30	10 56	35 29
18 25 0	14 15	36 9
18 27 30	17 10	36 41
18 30 0	19 50	37 8

Points on the limits of the total shadow.

Northern Limit.		Southern Limit.	
East Longitude.	North Latitude.	East Longitude.	North Latitude.
5 53	32 14	8 5	34 0
9 57	36 9	11 49	34 49
13 24	36 51	15 2	35 27
16 25	37 24	17 52	35 58
19 9	37 52	20 27	36 23

29. From the Elements with Variation the following numbers are obtained :—

	19 ^a .	20 ^a .
Moon's Longitude.....	136° 1' 17".2	136° 38' 50".0
Moon's North Latitude.....	17 13.8	20 42.2
Moon's Right Ascension	138 37 59.7	139 16 23.5
Moon's North Declination	16 29 58.3	16 21 53.2

Greenwich Mean Solar Time of conjunction in R.A. —309, Aug. 14^d 19^h 45^m 2^s.

R.A. of Sun and Moon at conjunction 139° 6' 48".5.

Sun's North Declination at conjunction 16° 3' 5".5.

Moon's North Declination at conjunction 16° 23' 54".3.

Greenwich Mean Solar Time of Middle of General Eclipse 19^h 52^m 24^s.

Coordinates of Points on the central path of the shadow.

Greenwich Mean Solar Time.	East Longitude.	North Latitude.
h m s	5 56	35 21
18 20 0	9 55	36 16
18 22 30	13 22	36 58
18 25 0	16 23	37 32
18 27 30	19 7	38 0

The breadth of the shadow, as marked on an ordinary map, for the Elements with Variation, will not sensibly differ from that for Greenwich Unvaried Elements.

30. The results of these calculations are laid down on the first map accompanying this paper, Plate XII. The large dots show the two possible positions of AGATHOCLES. The strong line shows the central path of shadow, and the faint dotted lines show the limits of the dark shadow, from Greenwich Unvaried Elements. The interrupted line shows the central path of shadow from Elements with Variation (the term "Variation" signifying an increase of the Mean Argument of Latitude by 0°.20). The other lines show the positions of the central path of shadow necessary for satisfying respectively the four following conditions:—1. that the Northern boundary of the shadow will touch the Southern position of AGATHOCLES; 2. that the Northern boundary will touch the Northern position; 3. that the Southern boundary will touch

the Southern position; 4. that the Southern boundary will touch the Northern position. The shaded irregular ovals are intended merely to show the breadth of the dark shadow in the direction transverse to the path of shadow, without any relation to the extent of shadow at any one instant in the direction of the path.

31. From an inspection of this map, with some geographical measures, the following conclusions will be easily deduced:—

(a.) With Unvaried Elements, the dark shadow passes over the Southern position of AGATHOCLES, but not over the Northern position.

(b.) In order that the eclipse may be total at the Southern position, the change of Mean Argument of Latitude must be included between those corresponding to Condition 1 and Condition 3, or between

$$-0.55 \times \text{Variation and } +1.14 \times \text{Variation.}$$

(c.) In order that the eclipse may be total at the Northern position, the change must be included between those corresponding to Condition 2 and Condition 4, or between

$$+0.68 \times \text{Variation and } +2.38 \times \text{Variation.}$$

The latter is the system of limitations which, for reasons already explained, I am disposed to adopt.

32. Assuming the uncertainty in the circumstances of an eclipse to be due entirely to uncertainty in the place of the moon's node, these statements supply us with an easy method of correcting (within certain limits) the elements for any other eclipse, according as we adopt one or the other position of AGATHOCLES. It is only to be remarked that if, in making the application to another eclipse, we adopt in the calculations for that eclipse (as will be convenient) a Variation of $0^{\circ}20'$, then the factors corresponding to these Conditions must be increased in the same proportion as the interval of time backwards from the present age (when the place of the node is well known) to the age of the eclipse in question. Thus in applying them to the eclipse of THALES we must increase the factors by about $\frac{1}{4}$ th part.

SECTION IV. *Eclipse of THALES, as recorded by HERODOTUS.*

33. The account of this eclipse given in the first book of HERODOTUS is in substance as follows. "Upon the refusal of ALYATTES (king of the Lydians) to give up some Scythian fugitives to CYAXARES (king of the Medes), the Lydians and the Medes were at war for five years; during which the Medes often defeated the Lydians and the Lydians often defeated the Medes; they had also in this war a sort of night-battle; and while they were still carrying on the war with equal success, and met for battle in the sixth year, it happened that on the battle being joined the day suddenly became night. THALES the Milesian predicted to the Ionians that this change would happen, fixing beforehand this very year, in which the change did occur. The Lydians and the Medes, when they saw that it was night instead of day, ceased from fighting, and on both sides endeavoured more anxiously to obtain peace.

The persons who brought them together were SYENNESIS the Cilician and LABYNETUS the Babylonian."

34. It is to be remarked that this war was one of a different character from that which was subsequently undertaken by CRÆSUS and which ended in his ruin. The war of ALYATTES was a struggle between two nations, in which the Medes apparently made the first movement; and though it is impossible to say how far in so long a time the places of conflict may have been shifted, yet it is likely that they would always have reference to the great lines of military communication between the two warring countries. The war of CRÆSUS, on the other hand, was undertaken to obtain possession of the province of Cappadocia. It must be remembered that the limits of this province, in the geography of HERODOTUS, were very different from those assigned to it in later times, and which are generally traced in our maps. With him, the province (then an independent kingdom) of Cilicia included both banks of the upper part of the Halys (and therefore extended very much further north than in later times); then the Matieni occupied the right bank of the river, and the Phrygians the left; then, from their boundary to the mouth of the Halys, the Cappadocians (who he says were called Syrians by the Greeks) occupied the right bank of the Halys (thus including what was afterwards the kingdom of Pontus) and the Paphlagonians the left. Thus, in the Cappadocian enterprise of CRÆSUS (which, as the attack on Pteria shows, was principally directed against the inhabitants of the coast), it was necessary to pass the Halys near its mouth, and with the difficulties described by HERODOTUS; but in the Median war of ALYATTES there was not necessarily any movement so far north. The circumstance that the armies in this eclipse-battle were accompanied by the forces of their principal allies, and that the kings were present in person ready to make a treaty, shows that it was no skirmish of detachments, but a meeting of the main armies. It will be well therefore to consider in what part of the country such armies were likely to meet. I am indebted to M. PIERRE DE TCHIHATCHEFF and W. J. HAMILTON, Esq., for much of the information on which the following remarks are founded.

35. Asia Minor is bounded on its eastern side by a wide-spreading cluster of mountains, which, apparently, presents to the west an unbroken front, extending from the Euxine Sea to the Gulf of Issus; and on its southern side by a narrow range of mountains joining the former near Issus. The difficulties of passing the eastern mountains appear to be great. There is one road leading from Erzeroum by Sebaste or Sivas towards Cæsarea, and another road nearly parallel to this, thirty or forty miles S.E. of it; but both are rough and pass through very extensive tracts which provide little food. A rough road leads in the S.E. direction from Cæsarea by El Bostan. The best road appears to be that which leads from Sivas to Guroun, and then accompanies one of the feeders of the Euphrates by Melitene or Malatieh. In the southern mountains, the best pass towards the shore of the Mediterranean is that of Tarsus, leading thence by Issus to Antioch or Aleppo. Mesopotamia has only

once (I believe) been invaded from the Euxine Sea; namely, by an army directed by the Byzantine emperor HERACLIUS (A.D. 623), which landed at Trebizond and made its way through the mountains; returning however by the way of Issus. There is only one instance of an army marching along the north coast of Asia Minor, namely, that of the ten thousand Greeks in their return from the Anabasis; but this route was not adopted from choice; and the difficulties which they experienced show that it is not likely that a large army would willingly take that line. It would appear therefore that there are but two routes really practicable for armies; that of Melitene and that of Issus. At Melitene was fought the important battle, A.D. 572, between the Emperor TIBERIUS and CHOSROES NUSHIRVAN. One great battle, that between TIMUR and BAJAZET, A.D. 1402, was fought as far north as Ancyra; it was perhaps preceded by movements on the Melitene road. It is probable also that other marches have been made on the same line. But the far greater number of marches in both directions have been by the pass of Tarsus and the coast line to Issus. This was the route of the younger CYRUS; of ALEXANDER, although he marched from Ancyra; of VALERIAN and JULIAN; of SAPOR in marching from Armenia to the Cappadocian Cæsarea (for which the pass of Melitene would have appeared more direct); of the Crusaders in the first and second crusades; and of many other armies. When, in marching eastward, the valley of Antioch or the more open plains of the Euphrates are gained, it is difficult to define with the same strictness the probable march of a military force. The account of HERODOTUS however conveys the impression that the eclipse-battle took place in or very near to Asia Minor.

36. I conceive therefore that we are limited, as to the battle field, to the country within no great distance of the line from Sardes to Melitene; that it may have been anywhere south of that line, especially near Issus, but that it cannot have been far north of it; and that it cannot have been far east of Issus.

37. The approximate examination of the eclipses which could pass near this tract is very greatly facilitated by the tables in the *Art de vérifier les dates*, but still more by the calculations of Mr. BAILY and Mr. OLTMANN. It is only necessary to observe that the correction of the moon's elements increases the argument of latitude (by which the track of the shadow at every eclipse in ascending node will be thrown three or four degrees northward, and that at every eclipse in descending node will be thrown as much southward); and that it increases the secular equation of anomaly, and thus increases the moon's longitude at every perigee eclipse (by which the track of the shadow in every eclipse will be thrown several degrees to the east). Thus I have examined every total eclipse in Mr. OLTMANN'S table, extending from B.C. 631 to B.C. 585; and find only one (namely, that of B.C. 585, May 28*) which can have passed near to Asia Minor; that of B.C. 610, Sept. 30, which was adopted by Messrs.

* The first publication of results relating to the eclipse of B.C. 585, derived from careful calculations on good elements, so far as I know, was that by J. R. HIND, Esq., in the *Athenæum* for 1852, August 28, during the preparation of the present memoir.

BAILY and OLTSMANN, is now thrown north even of the sea of Azov. I have likewise formed the first approximate elements of the eclipses from B.C. 630 to B.C. 576, by the use of M. LARGETEAU's very convenient tables inserted in the *Additions to the Connaissance des Temps*, 1846, and am led to the same conclusion.

38. I shall now proceed with the computation of the eclipse of B.C. 585, May 28. The elements are formed precisely in the same manner as those for the eclipse of AGATHOCLES. The following are the primary numbers for the moon, for -584 (or B.C. 585), May 28, 2^h, Greenwich Mean Solar Time.

	<i>u</i>	<i>x</i>	<i>l</i>	<i>z</i>	<i>y</i>
DAMOISEAU'S Elements.....	67°37'53.9	388°09'70	1°09'41	200°9'173	4°6'101
Greenwich Corrections	-29'11.2	-06'26	-29'11		+1'723
Greenwich Unvaried Elements.....	67°08'527	388°03'44	0°0'30	200°9'173	4°7'824
Elements with Variation	67°08'527	388°03'44	0°0'30	200°9'173	4°9'824

With the Greenwich Unvaried Elements, the following places are obtained:—

	2 ^h	3 ^h
Obliquity of Ecliptic.....	23° 45' 55.6	
Sun's Longitude	59° 34' 37.0	59° 37' 0.0
Sun's Right Ascension	57° 18' 49.9	57° 21' 18.7
Sun's North Declination	20° 20' 5.3	20° 20' 36.4
Sun's Semidiameter	15' 44.4	
Moon's Longitude.....	59° 23' 41.1	60° 1' 34.9
Moon's North Latitude.....	17' 4.5	20' 34.8
Moon's Right Ascension	57° 3' 27.9	57° 42' 11.2
Moon's North Declination	20° 34' 21.8	20° 46' 1.1
Moon's Equatorial Horizontal Parallax	61' 18.8	
Moon's Geocentric Semidiameter.....	16' 44.8	
Sun's True Right Ascension, in time	3 ^h 49 ^m 15 ^s .32	
Sun's Mean Right Ascension, in time.....	3 ^h 58 ^m 23 ^s .04	

From these, the following numbers are deduced:—

Greenwich Mean Solar Time of conjunction in R.A. - 584, May 28^d 2^h 25^m 27^s.

R.A. of Sun and Moon at conjunction 57° 19' 53".0.

Sun's North Declination at conjunction 20° 20' 18".5.

Moon's North Declination at conjunction 20° 39' 18".3.

Greenwich Mean Solar Time of Middle of General Eclipse. 2^h 15^m 29^s.

From which the following coordinates of points are obtained:—

Points on the central path of the shadow.

Greenwich Mean Solar Time.	East Longitude.	North Latitude.	Sun's Zenith Distance.
h m s			
3 48 0	23° 1'	39° 46'	71° 27'
3 49 0	24° 56'	39° 14'	73° 11'
3 50 0	27° 3'	38° 37'	75° 8'
3 51 0	The formulæ fail.		
3 52 0	32° 26'	36° 54'	80° 5'
3 53 0	36° 34'	35° 29'	83° 54'

Points on the limits of the total shadow.

Northern Limit.		Southern Limit.	
East Longitude.	North Latitude.	East Longitude.	North Latitude.
24 35	40 29	21 37	39 2
26 38	39 53	23 25	38 32
28 56	39 12	25 24	37 58
31 39	38 20	27 37	37 18
35 9	37 9	30 14	36 29
42 14	34 36	33 33	35 22

39. From the Elements with Variation, the following numbers are obtained:—

	2 ^a .	3 ^a .
Moon's Longitude	59° 23' 38".6	60° 1' 32".3
Moon's North Latitude	18 1.1	21 31.4
Moon's Right Ascension	57 3 12.0	57 41 55.5
Moon's North Declination	20 35 16.4	20 46 55.7

Greenwich Mean Solar Time of conjunction in R.A. — 584, May 28^d, 2^h 25^m 53^s.
 R.A. of Sun and Moon at conjunction 57° 19' 54".1.
 Sun's North Declination at conjunction 20° 20' 18".7.
 Moon's North Declination at conjunction 20° 40' 18".1.
 Greenwich Mean Solar Time of Middle of General Eclipse 2^h 15^m 24^s.

Coordinates of Points on the central path of the shadow.

Greenwich Mean Solar Time.	East Longitude.	North Latitude.
h m s		
3 48 0	24 36	40 22
3 49 0	26 39	39 46
3 50 0	28 57	39 5
3 51 0	31 41	38 12
3 52 0	35 13	37 1

It will be remembered that the term "Variation" here has the technical sense of "increase of the mean argument of latitude by 20'." And it must be remarked that, to find the positions of the path of shadow which correspond respectively to Conditions 1, 2, 3, 4 in the eclipse of AGATHOCLES, the effects of "Variation" must be multiplied by —0.63, +0.78, +1.31, +2.72.

40. In the second map which accompanies this paper (Plate XIII.), I have shown by a strong line the central path of the shadow corresponding to Unvaried Elements, and by an interrupted line the path corresponding to Elements with Variation of 20' in the Argument of Latitude; and also, by finer continuous lines, the paths corresponding to Conditions 1, 2, 3, 4 in the eclipse of AGATHOCLES; together with those that correspond to Central eclipse on the Southern position of AGATHOCLES and

Central eclipse on the Northern position of AGATHOCLES. The oval whose centre is on the line of Unvaried Elements is intended only to show the breadth of the shadow, which will be sensibly the same for each of the positions of central path. On examining these, the following remarks will at once suggest themselves.

41. If the centre of the shadow followed the line of Condition 1, the shadow would obscure no open country except a very small distance north-east of the Pisidian Mountains; and even there the obscurity would be short. The western towns however from Halicarnassus to Pergamum would be shaded.

If the centre followed the line corresponding to a central eclipse for the Southern position of AGATHOCLES, the shadow would be almost central at what was afterwards Antioch, Celenæ and Pergamum; and there would be a great total eclipse at Issus and Tarsus, and on all the western towns from Ephesus to Troy; and the southern plain of Iconium would be in the shade. But Cæsarea, Melitene, Ancyra, would be in light.

If the centre followed the line of Condition 3, Issus, Tarsus, and a great length of the southern road, would be covered; but the shadow would not extend to Cæsarea, Ancyra, or Melitene. Sardes would now be out of the shadow.

If the centre followed the line corresponding to central eclipse for the Northern position of AGATHOCLES, Issus, Tarsus, Cæsarea, Iconium, would be within the shadow; Melitene, Pergamum, Ancyra, would be in light. Generally, the centre of the peninsula would be in shade.

If the central point followed the line of Condition 4, Melitene would be shaded; Cæsarea and Ancyra would be nearly in the middle of the shadow; and the northern plains would be covered; but the plain of Iconium and the whole western coast south of Lampsacus would be free from shade.

42. Any one of these tracks of the shadow (perhaps excepting that of Condition 1) is compatible with a conceivable place of engagement. In balancing the probabilities, we must in some measure be guided by the extent of ground proper for large military operations which the total shadow would cover. Judging thus, I should fix on a course between that of Condition 3 and that corresponding to central eclipse at the North position of AGATHOCLES as most probable. This selection, it will be remarked, excludes the possibility of AGATHOCLES being at the South position; and therefore, if adopted, would decide absolutely that AGATHOCLES sailed on the north side of Sicily.

43. If in conformity with this selection we suppose that the Unvaried Argument of Latitude ought to be increased by $1.53 \times$ variation of $20'$, or by $30' 60''$, and if we remark that the interval of time from 1782, when DAMOISEAU's epoch is nearly correct, is -23.66 centuries, it will appear that the secular motion of argument of latitude ought to be diminished by $1' 29''$ or $42''$ nearly. Assuming our motion of mean longitude to be correct, the same correction ought to be applied to the regression of the node. This makes the secular regression of the node for a Julian century equal to $134^{\circ} 8' 13''$, which is rather less than the smallest of those found by other inves-

tigators. If we had supposed the shadow to follow the line of Condition 3, the number would have agreed very closely with those smallest numbers.

44. In terminating this section, I may remark on the causes of uncertainty which yet remain in the theoretical calculations. The mean motion of the moon is determined by observations which are more completely free from constant error than almost any other observations applicable to one element; and I have no doubt of its extreme correctness. The great theorists of the present age, however, do not agree very closely in the value which they ascribe to the coefficient of the term in the moon's longitude depending on the square of the time. The present motion of the moon's perigee is determined with a certainty only inferior to that of mean motion. But Professor HANSEN in his last published investigations has proposed to alter the coefficient of the squares of centuries in the place of perigee by $3''$, which will affect the moon's longitude in these eclipses by more than $3'$, and will produce an effect opposite to that of regression of node in the eclipse of AGATHOCLES, but combining with it in the eclipse of THALES. The determination of the movement of the node from observation is liable to uncertainty only from the negligence of the Greenwich observers in the last century, who did not carefully observe the zenith distance of the moon's limb at the precise instant when it passed the meridian; and the effect of this error may be considerable. The theoretical term depending on the square of the time appears (if we may so infer from the consent of the investigators) to be well determined.

45. I conclude therefore that the terms to which at present it is most desirable that the attention of theorists should be directed, are those in the mean longitude and in the longitude of perigee depending on the square of the time. A careful discussion of eclipses will then supply, what meridional observations at present are hardly able to supply with the requisite accuracy, the motion of the node.

SECTION V. *Eclipse recorded by the Persian Historians.*

46. In Sir JOHN MALCOLM's History of Persia, Chapter VII., is a comparison of the Persian history, as recorded in Persian poetry (founded undoubtedly on authentic history, though with many changes and very great omissions), with that recorded by Greek writers. It appears that the KAI KAOOS of the Persians is the same as the ASTYAGES of the Greeks, or that the events of his reign are those of both ASTYAGES and CYAXARES; and Sir JOHN MALCOLM adds, "the most remarkable agreement is in the expedition of KAI KAOOS to Mazanderam. We are told by the Persian poet that in a battle which was fought in that province, the prince and his army were struck with a sudden blindness, which had been foretold by a magician."

47. In the range of years through which my examination has extended, there was no total eclipse in Mazanderam, and only two which could be visible in the eastern dominions of Persia. One was the eclipse of B.C. 610, Sept. 30; of which the central path, as computed by J. R. HIND, Esq., from elements not very unlike mine (which,

as well as every part of the results, Mr. HIND has most obligingly communicated to me), passed over Königsberg, Astrakhan, and Khiva. The other is that of *a.c.* 603, May 17, which crossed the Persian Gulf in a north-easterly direction.

48. I imagine therefore that if this be a record of a total eclipse (which I see no sufficient reason for doubting), it relates to the same eclipse as that recorded by HERODOTUS. It appears, from Sir JOHN MALCOLM's remarks on the Persian historical traditions in general, that the names of provinces are in many instances given erroneously.

SECTION VI. *Eclipse of XERXES.*

49. In the spring of the same year in which the battle of Salamis was fought (to which the date *a.c.* 480 is usually assigned), there occurred a phenomenon which is thus described by HERODOTUS, Book vii. "With spring, the army [of the Persians], being ready, set out from Sardes on its march to Abydos; and as it was setting out, the sun leaving his seat in heaven was invisible, when there were no clouds but a perfectly clear sky; and instead of day it became night. XERXES, who saw this and heard about it, felt some anxiety, and inquired of the Magi what the appearance portended; they replied that the deity prognosticated to the Greeks the desertion of their cities; saying that the sun was the prognosticator for the Greeks, the moon for the Persians. When XERXES heard this he was very joyful, and proceeded on his march."

50. This account, interpreted as a record of a total eclipse of the sun, has given great trouble to chronologers, and not without reason. The only solar eclipse which it is worth while even to examine is that in the morning of the 19th of April, *a.c.* 481. The numbers computed from Greenwich Unvaried Elements are as follows:—

For —480, April 18, 16^h, Greenwich Mean Solar Time.

	<i>u.</i>	<i>x.</i>	<i>l.</i>	<i>z.</i>	<i>y.</i>
DAMOISSEAU'S Elements	21 ^f 90139	44 ^f 9528	397 ^f 9207	156 ^f 6612	191 ^f 9616
Greenwich Corrections	—27849	—0599	—2785		+1648
Greenwich Unvaried Elements.....	21-62290	44-8929	397-6422	156-6612	192-1264

	16 ^h .	17 ^h .
Obliquity of Ecliptic.....	23° 44' 56".7	
Sun's Longitude	22° 47' 45".1	22° 50' 9".1
Sun's Right Ascension	21° 2' 27".8	21° 4' 42".9
Sun's North Declination	8° 58' 37".8	8° 59' 31".4
Sun's Semidiameter	15' 48".2	
Moon's Longitude	22° 37' 22".9	23° 14' 5".5
Moon's North Latitude	19° 34".8	16° 12".0
Moon's Right Ascension	20° 45' 16".4	21° 21' 1".9
Moon's North Declination	9° 12' 51".7	9° 23' 31".5
Moon's Equatorial Horizontal Parallax	60' 19".3	
Moon's Geocentric Semidiameter.....	16' 28".6	
Sun's True Right Ascension, in time	1 ^h 24 ^m 9 ^s .85	
Sun's Mean Right Ascension, in time.....	1 ^h 26 ^m 7 ^s .57	

Greenwich Mean Solar Time of conjunction in R.A.	—480, April 18 ^d , 16 ^h 30 ^m 47 ^s .
R.A. of Sun and Moon at conjunction	21° 3' 37".4.
Sun's North Declination at conjunction	8° 59' 5".0.
Moon's North Declination at conjunction	9° 18' 20".0.
Greenwich Mean Solar Time of Middle of General Eclipse	16 ^h 21 ^m 18 ^s .

And from these,

Beginning of Central Eclipse on the Earth	14 ^h 41 ^m 47 ^s .
in Longitude 49° 17' East, Latitude 1° 25' North.	
Central Eclipse at Noon	16 ^h 30 ^m 47 ^s .
in Longitude 111° 49' East, Latitude 27° 49' North.	
End of Central Eclipse on the Earth	18 ^h 0 ^m 51 ^s .
in Longitude 173° 13' East, Latitude 34° 2' North.	

51. If a diagram is constructed to exhibit the path of the shadow in this eclipse over the earth, and if it is remarked that the longitude of Sardes is about 28° East, it will be found that there could not be even a partial eclipse for Sardes, the whole penumbra having entered completely upon the earth before sunrise at Sardes. Nor, if the calculations above are correct (as I have great reason to believe), does it appear possible by alteration of secular movements to make an eclipse visible at Sardes. For if the moon's longitude were diminished, to make this eclipse possible, it must also be diminished in B.C. 585, and that would make the eclipse of THALES impossible, as the moon would not then have entered upon the sun's disk before sunset.

52. Abandoning then the idea of explaining this account by a solar eclipse, I have examined into the possibility of referring it to some other phenomenon. First, I cannot doubt that there was something unusual and alarming, as the solemn consultation of the Magi by XERXES seems to have been a matter of notoriety. Secondly, HERODOTUS repeatedly expresses himself doubtful on matters of detail which occurred during the movements of XERXES on the eastern side of the Ægean sea. Thirdly, the notion that the Sun was the peculiar divinity of the Greeks and the Moon that of the Persians, is entirely opposed to all that we know of the religious ideas of the Persians generally, or of XERXES in particular. For instance, when XERXES was preparing to cross the Hellespont, he waited for the rising of the Sun, and then addressed to the Sun his prayers for success. The Greeks however appear to have attached great importance to the appearance of the Moon, as is evident from their terror, and its calamitous consequences, at the lunar eclipse in the Syracusan war (THUCYDIDES, book vii.). The reply of the Magi therefore, which (as given by HERODOTUS) is, on the face of it, absurd, would seem to be much more plausible if we suppose that the information received by HERODOTUS was wrong in one particular, and that the observation in question was an eclipse of the moon, instead of the sun.

53. Now there was an eclipse of the moon on the morning of the 14th of March, B.C. 479, which answers well to the conditions of the history. The elements of computation are, for —478, March 13, 15^h, Greenwich Mean Solar Time.

	w.	x.	y.	z.	u.
DAMOISREAU'S Elements	$181^{\circ}75'04$	$118^{\circ}90'84$	$197^{\circ}77'83$	$116^{\circ}62'27$	$392^{\circ}64'88$
Greenwich Corrections	$-27'849$	$-05'99$	$-27'85$		$+16'48$
Greenwich Elements Unvaried.....	$181^{\circ}47'855$	$118^{\circ}84'85$	$197^{\circ}49'98$	$116^{\circ}62'27$	$392^{\circ}81'36$

	14 ^h .	15 ^h .
Sun's Longitude	$347^{\circ}26'10\cdot7$	$347^{\circ}28'37\cdot2$
Moon's Longitude	$167^{\circ}23'0\cdot0$	$167^{\circ}55'2\cdot2$
Moon's South Latitude	$12^{\circ}10\cdot4$	$9^{\circ}12\cdot8$

The opposition in longitude occurred therefore at 14^h 6^m 28^s Greenwich Mean Solar Time, or about 15^h 59^m Sardes Mean Solar Time; and the Moon's South Latitude was $11^{\circ}51'1''$; which would be reduced to about $10^{\circ}35''$ by the corrections at which I arrived in Section IV. It was therefore a total eclipse, nearly central (the moon's limb being at least $16'$ within the inner boundary of the penumbra), and it is probable that the moon disappeared completely, and was lost for nearly two hours.

54. I think it extremely probable that this really was the eclipse to which the account of HERODOTUS refers. But for its adoption it is necessary to bring down the date of the battle of Salamis one year later than in the chronology generally received.

G. B. AIRY.

Royal Observatory, Greenwich,
1852, December 10.

VIII. *On the Dissolution of Urinary Calculi in dilute Saline Fluids, at the Temperature of the Body, by the aid of Electricity.*

By HENRY BENCE JONES, M.D., F.R.S., Physician to St. George's Hospital.

Received October 12,—Read December 16, 1852.

IN the year 1842 I published a statement of some of the attempts which had been made to dissolve urinary calculi. I did not then know that MM. PÉREVOST and DUMAS had made any researches on this subject. Some time afterwards I was told that they had used electricity, but I could not obtain the reference to their original paper; as no practical use was made of their experiments, I did not search for the record of them until after my own experiments were completed.

In 1845 I first devised the investigation which from circumstances I did not commence until 1848, when I made some experiments on the solution of uric acid calculi.

It appeared to me not unlikely that when a solution of nitrate of potassa was made to divide into potassa and nitric acid by means of electricity, if a piece of uric acid were held between the electrodes of the galvanic battery, then the potassa at the negative electrode and the nitric acid at the positive electrode would both act on the calculus.

Thus I expected that the fluid about the negative electrode would dissolve the uric acid, whilst the fluid about the positive electrode would decompose the uric acid. I considered that no great excess of acid or alkali would result, because after passing round the calculus I expected they would recombine, re-forming a neutral solution.

The battery used consisted of six pairs of GROVE's plates. The action was continued about four hours. There was an effect produced on the uric acid at the negative pole, but no very decided result was obtained.

A second experiment gave so little encouragement that for the time the experiments were discontinued.

In the year 1851 I proposed again trying, under the same circumstances, the effect of nitre on uric acid, of sulphate of potash on oxalate of lime, and of lactate of soda on phosphatic calculi. I was unable to make any experiments until July 1852, when, through the kindness of Dr. FARADAY, I was allowed to have the use of the batteries at the Royal Institution, and I had the valuable assistance of Mr. ANDERSON, by which the perfect action of these batteries was ensured.

SECTION I. *On the Solution of Uric Acid Calculi.*

After a few preliminary experiments, on the 3rd of July the first experiment was made; a piece of very compact uric acid calculus was put into a saturated solution of nitrate of potassa, between the electrodes of a battery consisting of ten pairs of Grove's plates. The fluid soon boiled; the action was continued between three and four hours, when the calculus was removed. It was found to be so deeply corroded and worn on both sides, that apparently nearly half the piece of uric acid calculus was gone (see Plate XIV. fig. (a) and (b)). The solution of nitre was boiling the whole time. On the removal of the stone a few drops of the liquid were evaporated, and the evidence of uric acid was most distinct. The fluid was evaporated to dryness, treated with a drop or two of nitric acid, again evaporated to dryness, and exposed to the fumes of ammonia. The characteristic reaction of uric acid was obtained.

Hence, by this qualitative experiment, it was certain that at a boiling temperature uric acid between the electrodes could be dissolved in a saturated solution of nitre.

The question immediately arose, what effect would be produced when the solution of nitre was diluted, and when the temperature was about that of the human body.

Experiment 2.—The number of plates in the battery was reduced to one-half in order that the temperature might rise less rapidly. Only five pairs of plates were used. Ten cubic inches of saturated solution of nitre were mixed with thirty cubic inches of distilled water; the specific gravity of the mixture was 1029. The temperature was kept low by changing the solution in which the calculus and the electrodes were placed. Each glass contained nearly a pint of the solution of nitre. The piece of calculus weighed 48 grains, and it consisted of very compact uric acid.

The action began at 9^h 45^m A.M. and was continued to 1 P.M., the temperature of the solution being between 65° and 108°.

The action recommenced at 2^h 10^m P.M. and was continued to 5 P.M., the temperature being between 65° and 112°.

Total time 6^h 5^m.

The calculus was then removed and perfectly dried in a water-bath. It was not washed with water, so that some nitre probably remained on the calculus; when perfectly dry the uric acid weighed 37 grains. Hence, in six hours at least, 11 grains of uric acid were dissolved. The solution appeared to take place chiefly at the negative pole. The nitre solution became very slightly alkaline.

Experiment 3.—I was then anxious to know whether a larger piece of calculus and increased power of the battery would give me a satisfactory result; the piece of calculus, weighing 113 grains, consisted chiefly of uric acid, but the nucleus was oxalate of lime. The battery consisted of ten pairs of plates, and the electrodes and solution were the same as in the last experiment.

The action began at 10^h 10^m A.M. and was continued to 1 P.M., the temperature of the solution being between 67° and 104°.

The action was recommenced at 2^h 10^m P.M. and was continued to 5^h 30^m P.M., the temperature being between 70° and 108°.

Total time 6^h 10^m.

The calculus was then removed, dried in a water-bath, after which it weighed 99 grs., so that in six hours about 14 grains of uric acid were dissolved. The external surface of the calculus which had been in contact with the negative electrode was corroded, whilst the internal surface, which consisted of oxalate of lime and which had been in contact with the positive electrode, was apparently very slightly acted on. The solution of nitre was much discoloured, and on evaporating, even a few drops, uric acid was immediately detectable.

Experiment 4.—A whole uric acid calculus, weighing 111 grains, was put in a solution of nitre of specific gravity 1024.5. Ten pairs of plates were employed.

The action began at 9^h 35^m A.M. and was continued to 1^h 5^m P.M., the temperature of the solution being between 67° and 100°.

The action recommenced at 2^h 10^m P.M. and was continued until 5 P.M., the temperature being from 70° to 102°.

Total time 6^h 20^m.

The calculus when dried weighed 95 grains, so that in little more than six hours 16 grains of uric acid were dissolved. The solution took place almost entirely at the negative or alkaline electrode. The solution of nitre became slightly alkaline.

I was now desirous of trying whether the form of the electrodes at that part which was in contact with the calculus made any perceptible difference. Instead of a piece of thick platinum wire flattened, of the exact size and form of fig. 2, the wire was not flattened but only bent back. The breadth and length of the electrodes was nearly the same as in the previous experiments, but the surface in the former case was continuous, in the latter interrupted by a small space between the bent portions of the wire.

Experiment 5.—A whole calculus, consisting chiefly of uric acid, with a thin external coating of oxalate of lime with a little phosphate of lime, was employed. It weighed 57 grains. At first the lower electrode was made positive, and the upper electrode negative. The power of the battery and the strength of the solution were the same as in the last experiment.

The action began at 9^h 30^m A.M. and was continued to 1 P.M., the temperature of the solution being between 64° and 110°.

The action recommenced at 2 P.M., the electrodes were then reversed, the negative being below, and the action was continued to 5 P.M.; the temperature was from 70° to 120°.

Total time 6^h 45^m.

When dry the stone weighed 45 grains, so that the loss was 12 grains. Very little

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action was apparent on the calculus until the alkali was evolved from below. At five o'clock a voltameter interposed in the circuit showed that 2 cubic inches of mixed gases were produced in three minutes, but at this time the action of the battery was going down.

No deduction could be made as to the influence of the different forms of the electrodes.

Being anxious to know whether increased power of the battery and a better arrangement for keeping the temperature about that of the human body would be attended with advantage, another uric acid calculus was taken. It was very flat, and had been divided. The half used weighed $107\frac{1}{2}$ grains. The electrodes were those used in the first experiments. The portions not in contact with the calculus were covered with glass. The negative electrode was on the external surface of the stone, and was below. The solution consisted of the same strength of nitre as before, but to three pints of the solution eight ounces of urine were added; this being taken as the largest quantity likely to be secreted during the time that the experiment was likely to be continued in the human body. The battery consisted of twenty pairs of Grove's plates. By means of a siphon leading from the vessel in which the calculus was placed, and by means of a reservoir with a stopcock, a continuous current of the solution was allowed to flow around the stone; thus the temperature could be most easily regulated.

Experiment 6.—The action began at 9^h 28^m A.M., the temperature of the solution then being 64°.

At 9^h 55^m A.M., the voltameter showed that 1 cubic inch of mixed gases was evolved in thirty-five seconds.

The action was stopped at 12^h 45^m.

Total time 3^h 17^m.

The calculus when dry weighed 80 grains, so that the loss was $27\frac{1}{2}$ grains in three hours and a quarter. There was a deep red stain on the calculus when it was removed from the solution; this increased on drying, but it was more marked at the negative than at the positive electrode. The calculus was partially cut in halves. (See fig. 3.) On the negative electrode there was a deposit, which, when scraped off and examined, proved to be earthy phosphate, probably a deposit from the urine. The calculus itself on the surface, near the negative electrode, was made white from a slight deposit of earthy phosphate.

The current of the solution of nitre was continuous throughout, and the temperature was kept between 90° and 106°; when the action was stopped, the fluid was still slightly acid from the urine which was originally added.

No other experiments were made with uric acid calculi, so that no deductions have to be made for unsuccessful experiments.

The results may be thus arranged :—

- Exp. 1 lasted 4^h in saturated solution of nitre, temp. 212° with 10 pairs. Half dissolved.
 Exp. 2 lasted 6^h 5^m, $\frac{1}{4}$ nitre, $\frac{3}{4}$ water, average 109° with 5 pairs, 11 grs. dissolved.
 Exp. 3 lasted 6^h 10^m, $\frac{1}{4}$ nitre, $\frac{3}{4}$ water, average 101° with 10 pairs, 14 grs. dissolved.
 Exp. 4 lasted 6^h 20^m, $\frac{1}{4}$ nitre, $\frac{3}{4}$ water, average 100° with 10 pairs, 16 grs. dissolved.
 Exp. 5 lasted 6^h 45^m, $\frac{1}{4}$ nitre, $\frac{3}{4}$ water, average 106° with 10 pairs, 12 grs. dissolved.
 Exp. 6 lasted 3^h 17^m, $\frac{1}{4}$ nitre, $\frac{3}{4}$ water, average 98° with 20 pairs, 27 $\frac{1}{2}$ grs. dissolved.

So that uric acid calculi can be dissolved in a moderately dilute solution of nitrate of potassa at the temperature of the human body, by the aid of electricity, at the rate of from two to nine grains an hour.

SECTION II. *On the Solution of Oxalate of Lime Calculi.*

My original conjecture was, that sulphate of soda would prove the best solvent of oxalate of lime. Having however obtained so good an action on uric acid by a solution of nitre, I determined first to see what the effect of the same solution would be on oxalate of lime.

Experiment 1.—Half a small oxalate of lime calculus, weighing 42·5 grains, was taken. The battery consisted of only five pair of Grove's plates.

The solution of nitre was of the specific gravity 1028.

The action began at 9^h 25^m A.M., the temperature of the solution then was 65°.

The action was stopped at 4^h 35^m P.M., the temperature never rose above 90°.

Total time 7^h 10^m.

The calculus was then removed and perfectly dried; it then weighed 42 grains, so that in upwards of seven hours only half a grain was dissolved.

Experiment 2.—The same calculus, weighing 42 grains, was again placed in the same solution. The strength of the battery was doubled, being ten pairs.

The action began at 9^h 35^m A.M. and was continued to 1^h 15^m P.M., the temperature of the solution being between 70° and 115°.

The action recommenced at 2^h 30^m P.M. and continued to 5^h 50^m P.M., the temperature being between 75° and 104°.

Total time 7^h.

For the last two hours there was a great diminution in the power of the battery. The calculus when dry weighed 40 grains, so that in seven hours only 2 grains had been dissolved. It appeared that the divided surface was the one attacked; to this the positive electrode had been applied.

Experiment 3.—As the nitre solution had so little action, I proceeded to try the effect of a solution of sulphate of soda.

A solution of sulphate of soda was made, having the specific gravity 1034. The battery was of the same strength as in the previous experiment, and the same calculus was used, so that the comparative action of the solution of nitre and the solution

of sulphate of soda might be determined. The weight of the calculus then was 40 grains.

The action commenced at 9^h 10^m A.M. and was continued to 1 P.M., the temperature of the solution being between 64° and 104°.

The action recommenced at 2^h 5^m P.M. and was continued to 4^h 30^m P.M., the temperature being between 70° and 102°.

Total time 6^h 15^m.

The battery was in good condition throughout the day. When the calculus was dry it weighed 38 grains, so that in six hours and a quarter only 2 grains had been dissolved.

Comparing this with the previous experiment, there is no advantage in using the sulphate of soda instead of the nitrate of potash.

I proceeded to try whether a solution of chloride of sodium would be more efficacious than the sulphate of soda or nitrate of potash.

Experiment 4.—The calculus was taken which had already been used in the previous experiments, in order that the comparative result might be as conclusive as possible. The chloride of sodium was dissolved in undistilled water, and the specific gravity of the solution was 1053.5. The battery was of the same strength as before.

The action began at 9^h 30^m A.M. and was continued to 1 P.M., the temperature of the solution being between 68° and 108°.

The action recommenced at 2^h 15^m P.M. and was continued to 4^h 30^m P.M., the temperature being between 70° and 100°.

Total time 5^h 45^m.

During the whole action much chlorine gas was evolved. The stone was perfectly bleached in places. When dry it weighed 37 grains. In five hours and three quarters only 1 grain was dissolved; thus it was evident that a solution of common salt was inferior to a solution of Glauber-salt or of nitre. In consequence of these experiments, it appeared most desirable to try whether the solution of nitre could be made still more active, either by increasing the strength of the battery, by adding other substances to the solution of nitre, or by increasing the strength of the solution of nitre. For determining these questions the following experiments were made.

Experiment 5.—The battery was increased to double the number of plates; that is, to twenty pairs. The calculus was the same as before, and the solution of nitre had the specific gravity 1026.

The action began at 9^h 35 A.M. and was continued to 1 P.M., the temperature of the solution being between 67° and 130°.

The action recommenced at 2 P.M. and was continued to 4^h 45^m P.M., the temperature being between 70° and 100°.

Total time 6^h 10^m.

The action of the battery diminished at one o'clock, so that the full action was only 3^h 25^m.

The calculus when dry weighed 32 grains, so that the loss was 6 grains in 6^h 10^m. The solution was chiefly on the cut surface of the calculus, which was in contact with the positive electrode.

Thus it appeared that by increasing the intensity of the battery the solution of nitre acted more energetically. The calculus however was a very small one; it originally weighed 42½ grains only. One quarter of this however was dissolved.

Experiment 6.—The addition of other substances to the nitre solution was then tried; and first, phosphate of soda was employed. To each pint of the nitre solution two ounces of a saturated solution of phosphate of soda were added. A divided oxalate of lime calculus was taken which weighed 173 grains. The battery consisted of twenty pairs of plates.

The action commenced at 9^h 30^m A.M. and was continued to 12^h 49^m A.M., the temperature of the solution being between 66° and 110°.

The total time was 3^h 19^m.

The calculus when dry weighed 172 grains, so that in 3^h 19^m the loss was only 1 grain.

The calculus was exceedingly compact. There was no action whatever apparent at the negative or alkaline electrode which was on the upper and cut surface; the solution was of course alkaline throughout the whole time. The battery was in excellent action.

Experiment 7.—Two ounces of a strong solution of bichromate of potash were then added to each pint of the nitre solution. The same calculus was taken; it weighed 172 grains. Twenty pair of plates were again used.

The action began at 9^h 30^m A.M. and was continued to 12^h 45^m A.M., the temperature being between 65° and 120°.

The total time was 3^h 15^m.

When the calculus was dry it weighed 170 grains, so that the loss in three hours and a quarter was only 2 grains. Apparently this was taken from the cut surface of the calculus. The external surface, which was exceedingly hard, was not attacked, though it was in contact with the positive electrode. Finding thus no success I returned again to the nitre solution.

Experiment 8.—The solution of nitre was taken of double the strength of that which had previously been used, that is, one-half saturated solution and one-half distilled water. I was desirous of knowing whether the oxalate of lime would be more soluble in this than in a weaker solution; the battery and the calculus were the same as before. The calculus weighed 170 grains.

The action began at 9^h 30^m A.M. and was continued to 12^h 47^m A.M.; the temperature of the solution was between 64° and 110°.

Total time was 3^h 17^m.

When dry the calculus weighed 167½ grains, so that the total loss in three hours and seventeen minutes was only 2½ grains. There was very slight action on the

external surface of the stone which was in contact with the positive electrode. The cut surface around the nucleus appeared most acted on. The very frequent changes of the solution consequent on the resistance being lessened, led to the adoption of a continuous stream of solution.

Experiment 9.—It appeared from the last experiment, that by doubling the strength of the solution no corresponding increase in the action on the calculus occurred. To be still further convinced, the same calculus with the same battery was taken, and a solution was used of only half the strength of the previous one; that is, it was one-fourth saturated solution of nitre and three-fourths water, by means of a small tube siphon which carried off an ounce of fluid in twenty seconds, and a vessel with a stopcock which supplied fresh fluid. The temperature of the solution in which the calculus was kept remained about 92° , never rising higher than 96° .

The action began at 10 A.M., the temperature then was 64° .

The action was stopped at 12^h 50^m A.M., the temperature then was 92° .

Total time 2^h 50^m.

The calculus when dry weighed 165 grains, so that in two hours and fifty minutes of continuous action $2\frac{1}{2}$ grains only were dissolved. By comparing this experiment with the previous one, it is so far certain, that, by increasing the strength of the solution of nitre, no more action on the calculus was obtained. The next question was whether, by increasing the strength of the battery, the solution of the oxalate of lime would more rapidly take place.

Experiment 10.—The same calculus was used as before; it weighed now 165 grains. The solution of nitre was the same strength as in the last experiment. The battery was increased to forty pairs of GROVE's plates. The temperature was kept down by about six pints of nitre solution, and as the liquid passed out of the decomposing glass it was cooled by ice, and then returned to the reservoir. The temperature was thus kept throughout the whole experiment from rising above 102° ; one ounce ran through in five seconds.

The action began at 9^h 25^m A.M., the temperature then was 66° .

The action began at 9^h 47^m A.M. A voltmeter placed in the circuit showed that 1 cubic inch of mixed gases was evolved in twenty seconds.

The action was stopped at 12^h 25^m A.M., the temperature throughout was between 98° and 102° .

The total time 3^h.

The calculus when dried weighed 160 grains, so that the total loss in three hours was only 5 grains. There were very slight marks of action on the external surface. The hardness of this surface was between that of fluor and calc spar; Mr. TENNANT placed it for me at 3.5.

The current was continuous for three hours; the battery was in perfect order, though diminishing in action towards the end of the experiment.

The result of these experiments with oxalate of lime may be thus arranged:—

	h	m	Average temp.	Pairs of plates.	Grs. dissolved.
Exp. 1	lasted 7	10, in solution $\frac{1}{4}$ nitre, $\frac{3}{4}$ water . . .	90°	5	$\frac{1}{2}$
Exp. 2	lasted 7	10, in solution $\frac{1}{4}$ nitre, $\frac{3}{4}$ water . . .	104	10	2
Exp. 3	lasted 6	15, in sulphate of soda	101	10	2
Exp. 4	lasted 5	45, in common salt	102	10	1
Exp. 5	lasted 6	10, in nitre $\frac{1}{4}$, water $\frac{3}{4}$	108	20	6
Exp. 6	lasted 3	19, in nitre $\frac{1}{4}$ with phosphate of soda . . .	110	20	1
Exp. 7	lasted 3	15, in nitre $\frac{1}{4}$ with bichrom. of potassa . .	111	20	2
Exp. 8	lasted 3	17, in nitre $\frac{1}{2}$, water $\frac{3}{4}$	110	20	$2\frac{1}{2}$
Exp. 9	lasted 2	50, in nitre $\frac{1}{4}$, water $\frac{3}{4}$	92	20	$2\frac{1}{2}$
Exp. 10	lasted 3,	in nitre $\frac{1}{4}$, water $\frac{3}{4}$	100	40	5

Hence oxalate of lime calculi can be only very slowly dissolved in the solution of nitrate of potassa, which acts most energetically on uric acid calculi. The solution of the oxalate of lime is from $\frac{1}{2}$ a grain to 2 grains nearly an hour, so that the action is certainly four times as slow as in the case of uric acid calculi.

Such being the result when the calculus consisted of oxalate of lime only, it appeared desirable to determine what would be the effect when the calculus consisted of oxalate of lime with uric acid, or of oxalate of lime with the earthy phosphates.

Experiment 11.—Half an oxalate of lime calculus, which contained uric acid also on the external surface, weighed 258 grains. The solution of nitre, consisting of one-fourth nitre and three-fourths distilled water, with twenty pairs of plates, was employed.

The action began at 9^h 35^m A.M. and was continued to 1 P.M.; the temperature of the solution was between 64° and 120°.

Total time 3^h 25^m.

When dry the calculus weighed 254 grains, so that in three hours twenty-five minutes the loss was 4 grains. There was very little action on the calculus at the positive or acid electrode. The negative electrode was in contact with the external surface, and there the action had taken place.

Experiment 12.—The experiment with the same stone was repeated with the same solution and the same strength of battery. The solution was changed thirty-three times; the temperature was 110°; the time of action was three hours twenty-three minutes, in which the calculus lost 5 grains. There was very little action at the positive electrode, but very distinct at the negative electrode, which was on the upper and external surface.

Experiment 13.—Another small half calculus, consisting of oxalate of lime and uric acid, weighing 64 grains, was submitted to the action of twenty pairs of plates, whilst in a continuous stream of nitre of the same strength as before.

The action began at 9^h 32^m A.M., the temperature throughout was from 90° to 98°, and was stopped at 12^h 48^m A.M.

The total time 3^h 16^m.

When dry the stone weighed 60 grains, so that in three hours and a quarter 4 grains were dissolved.

The loss was chiefly on the cut surface, which was in contact with the negative or alkaline electrode. The surface was not regularly attacked, but some ridges were left, one very distinct, consisting of oxalate of lime. The external surface was below, and the acid appeared to have little or no effect on it.

It seemed from these experiments that uric acid and oxalate of lime together were more easily dissolved than oxalate of lime alone.

To determine whether oxalate of lime with the earthy phosphates was more soluble than oxalate of lime alone, the following experiments were made.

Experiment 14.—A whole calculus, consisting externally of oxalate of lime with phosphate of lime, weighing 186 grains, was acted on by twenty pairs of plates in the solution of nitre of the same strength as before.

The action began at 9^h 30^m A.M. and was continued to 1 P.M., the temperature of the solution being between 68° and 120°.

The action recommenced at 2^h 5^m P.M. and was continued to 3^h 35^m P.M., the temperature being between 70° and 100°.

The total time was 5^h. The last hour there was very little action of the battery.

The calculus when dry weighed 159 grains, so that in five hours 27 grains were dissolved; the result in this experiment was so much beyond my expectation that the same calculus was again submitted to the same reagents.

Experiment 15.—One grain having been removed for analysis the calculus weighed 158 grains.

The action began at 9^h 40^m A.M. and was continued to 12^h 45^m A.M., the temperature being between 65° and 130°.

The total time was 3^h 5^m.

When the calculus was dry it weighed 143½ grains, so that in three hours and five minutes the calculus lost 14½ grains.

The result of these experiments with oxalate of lime and uric acid, and with oxalate of lime and phosphates, may be thus arranged:—

Of lime and phosphat, may be thus arranged:—			Average temp.	Pairs of plates.	Grs. dissolved.	
	h	m				
Exp. 11 lasted	3	25,	in solution of nitre $\frac{1}{4}$, water $\frac{3}{4}$.	109	20	4
Exp. 12 lasted	3	23,	in solution of nitre $\frac{1}{4}$, water $\frac{3}{4}$.	110	20	5
Exp. 13 lasted	3	16,	in solution of nitre $\frac{1}{4}$, water $\frac{3}{4}$.	94	20	4
Exp. 14 lasted	5,		in solution of nitre $\frac{1}{4}$, water $\frac{3}{4}$.	103	20	27
Exp. 15 lasted	3	5,	in solution of nitre $\frac{1}{4}$, water $\frac{3}{4}$.	113	20	14½

Hence the oxalate and urate together can be slowly dissolved in the nitre solution, whilst the oxalate and phosphate together are very rapidly dissolved, namely, at the rate of from 4½ to 5½ grains an hour.

It appears then from these experiments that calculi consisting of oxalates with phosphates are very easily dissolved in a dilute solution of nitre at the temperature of the body by the aid of electricity; but that calculi consisting of oxalates with urates

are dissolved with difficulty, whilst calculi of oxalate of lime alone are acted on very slowly.

SECTION III. *On the Solution of Phosphatic Calculi and of Carbonate of Lime.*

Having found the solution of nitre so efficacious in its action on uric acid, and as it was at least as good if not better than any other solution in its action on oxalate of lime, it was most desirable that it should be found efficacious in its action on phosphatic calculi.

Experiment 1.—A piece of very hard phosphate of lime calculus, weighing 64 grains, was put into the nitre solution (one-fourth saturated solution and three-fourths distilled water). A battery of ten pairs of plates was first tried.

The action began at 9^h 15^m A.M. and was continued until 1^h 30^m P.M., the temperature being between 68° and 106°.

The action recommenced at 2 P.M. and was continued until 5 P.M., the temperature being between 70° and 106°.

The total time was 7^h 15^m.

The calculus when dry weighed 49 grains. Hence in seven hours and a quarter 15 grains were dissolved. The solution took place chiefly at the part in contact with the positive electrode.

Experiment 2.—A divided fusible calculus, weighing 85 grains, was subjected to the action of a battery of twenty pair. The solution of nitre was the same as before, but to three pints of the solution eight ounces of urine were added, being the quantity, or more than the quantity which would be secreted during the time of the experiment.

The action began at 9^h 27^m A.M.; the temperature of the solution was 70°.

The action was stopped at 10^h 40^m A.M.; the temperature was kept between 90° and 102°.

The total time was 1^h 13^m.

The calculus when dry weighed only 54 grains, so that the loss in one hour and thirteen minutes was 31 grains. The current was continuous throughout.

When the stone had been some time in the liquid, it was taken out with the electrodes still in contact. It then appeared that the stone had imbibed sufficient water to allow of the action going on through the wet stone without any liquid surrounding it. Hence probably the very rapid action which took place.

The negative electrode was found to be coated with earthy phosphates. The solution remained feebly acid throughout.

The positive electrode was below in contact with the external surface of the calculus which was cut through. See fig. 4.

On the Solution of Carbonate of Lime.

Experiment 3.—A piece of marble, weighing 170 grains, was put into the solution of nitre of specific gravity 1026. A galvanic battery of ten pairs of plates was employed.

The action began at 9^h 30^m A.M. and was continued until 1 P.M., the temperature being between 66° and 106°.

The action recommenced at 2 P.M. and was continued until 4 P.M., the temperature being between 70° and 108°.

The total time was 5½^h.

The piece of marble when dry weighed 142½ grains, so that in five and a half hours 27½ grains were dissolved. The solution became slightly alkaline; the marble was dissolved at the part in contact with the positive electrode. See fig. 5.

Experiment 4.—Another mass of marble, with the same battery, was tried in a solution of sulphate of soda, specific gravity about 1034.

The action began at 9^h 30^m A.M. and was continued until 1 P.M., the temperature being between 87° and 100°.

The action recommenced at 2 P.M. and was continued until 5 P.M., the temperature being between 100° and 106°.

The total time was 6½^h.

When the marble was dry it weighed 156½ grains. Hence in six hours and a half only 4½ grains of marble were dissolved. See fig. 6.

Without doubt the difficult solubility of the sulphate of lime was the cause that hindered the solution of the marble.

The contrast between the solution of nitre and the solution of Glauber-salt is very remarkable; the former being seven and a quarter times more efficacious than the latter.

The result of these experiments on the earthy phosphates and carbonate of lime may be thus arranged:—

	h m	Average temp.	Pairs of plates.	Grs. dissolved.
Exp. 1 lasted	7 15 in nitre solution	102°	10	15
Exp. 2 lasted	1 13 in nitre solution	96	20	31
Exp. 3 lasted	5 30 in nitre solution	104	10	27½
Exp. 4 lasted	6 30 in Glauber-salt	101	10	4½

Hence it is evident that calculi consisting of the earthy phosphates or of carbonate of lime can be most rapidly dissolved in a nearly neutral dilute solution of nitre, at the temperature of the human body, by the aid of the galvanic battery.

Conclusions.

Thus, then, by the aid of electricity, from 2 to 9 grains of uric acid calculi can be dissolved in an hour, whilst in the same time from 2 to 25 grains of phosphatic calculi can be dissolved in a neutral dilute solution of nitre at the temperature of the body. However, only from half a grain to 2 grains of oxalate of lime can be removed by the same means in the same time. Still, if the stone consists of oxalate with urate, from 1 to 2 grains may be dissolved in an hour; and if it consist of oxalate with phosphate, from 4½ to 5½ grains can be taken away in an hour.

Such at least are the results which can be obtained with calculi which have been long removed from the bladder and have been dried at 212° . The harder the calculi the greater the difficulty in dissolving them; and as previous to their removal from the bladder they are far less hard than they are after they have become dry, and as calculi which are wet may perhaps allow of the passage of the electricity through their substance, instead of only through the surrounding solution, there is good reason to expect that there will be less chemical difficulty in dissolving them in the bladder than out of it.

The skill which has been acquired in catching calculi in the bladder, the art of plating with platinum, the manufacture of vulcanized India rubber, will, with Mr. WEISS's help, I hope, enable me to state at some future time what can be done in the body as well as out of it. Looking back now from the results that have been obtained, it appears as if they might have been foreseen.

The uric acid and the phosphatic calculi are produced by the opposite states of over acidity and alkaliescence occurring in the urine for a considerable time. By means of the galvanic battery acting on a saline solution, acid and alkali can be made to appear at any desired spot, in any quantity and for any time. Thus these opposite conditions may be produced on the surface of the stone at the spot, and only at the spot, where the action is wanted to correct the consequence of too much acidity or alkaliescence; whilst around the place of action an almost neutral solution will remain in contact with the bladder without causing irritation.

I cannot conclude without stating what is due to MM. PRAVOST and DUMAS. Their note is printed in the *Annales de Chimie*, vol. xxiii. p. 202, 1823, *Sur l'Emploi de la Pile dans le traitement des Calculs de la Vessie**.

In this note the *mechanical action* of the mixed gases (evolved from the decomposition of water) on a fusible calculus is stated to have been 12 grains in 12 hours; twenty-five pairs of plates being used and recharged each hour.

The possibility of dissolving uric acid calculi is rejected, and no mention is made of calculi consisting of oxalate of lime.

In a postscript it is said that the addition of a "certain quantity" of nitre gave, with phosphatic calculi, a better result than when water alone was used.

M. DUMAS appears afterwards to have occupied himself with experiments on the possibility of employing currents of electricity within the bladder of living animals, and not with the determination of the *chemical action* of substances evolved by the galvanic battery on the surface of different kinds of calculi.

I am indebted to Dr. DU BOIS REYMOND for a reference to a paper, vol. i. p. 154, *Oesterreichische Medicinischen Jahrbucher* 1848, On the Effect of Galvanism on Urinary Calculi, by Dr. LUDWIG MELICHER of Vienna. He has employed the electric current for the electrolysis of the calculus itself, not for setting substances free which can act chemically upon it. He has made experiments on the living human body in two cases, it is said, with success.

* See also Dr. PARIS's *Pharmacologia*, vol. i. p. 231. ed. 6.

*Appendix to a paper on the Solution of Urinary Calculi in dilute Neutral Solutions,
by the aid of Electricity.*

By HENRY BENCE JONES, M.D., F.R.S., Physician to St. George's Hospital.

Received December 18, 1852.

SINCE the original paper was communicated to the Royal Society, it has appeared to me desirable to try whether more dilute solutions of nitrate of potash would have sufficient effect, when the strength of the battery and all other circumstances were the same as in the previous experiments.

The solution of nitre was therefore made to contain only 10 grains of nitre to an ounce of water; it had a specific gravity of 1015. Two ounces of urine were added to three pints of the nitre solution.

A fusible calculus, weighing when dry 352 grains, was placed in this solution between the electrodes of twenty pairs of Grove's plates.

The action commenced at 9^h 20^m A.M., the temperature being 50°.

The action was stopped at 12^h 45^m A.M., the temperature being 98°, so that the total time was 3^h 23^m. The calculus when dry weighed 324 grains, so that the total loss was 67 grains. See fig. 7.

Hence a solution of nitre of one-half the strength of that previously used is sufficient when decomposed to dissolve a fusible calculus at the rate of 19 grains an hour.

In order to observe the effect of reducing the strength of the battery when a dilute solution of nitre was employed, another calculus, consisting externally of phosphates, was placed in a solution of nitre, specific gravity 1015, and this was decomposed by ten pairs of Grove's plates.

The weight of the calculus when dry was 217 grains.

The action began at 9 A.M., the temperature then being 50°; at 12^h 45^m A.M. the action was stopped, the temperature being 98°.

The action was recommenced at 2 P.M., temperature 60°.

It was finally stopped at 4^h 45^m P.M., temperature 98°.

The total time was 6^h 30^m. The solution of nitre did not require to be changed during the whole time, the temperature remaining constant. After being acted on, the calculus when dry weighed 197 grains, so that the total loss was 20 grains.

Hence in twice the time not one-third of the effect shown in the previous experiment was obtained, though this was chiefly owing to the diminished power of the battery, yet the greater hardness of the calculus used in this last experiment no doubt influenced the result.

I then proceeded to try the effect of the dilute solution of nitre on a uric acid calculus.

A uric acid calculus, weighing 403 grains, was placed in a solution of nitre, specific gravity 1015, which was decomposed by twenty pairs of GROVE's plates.

The action commenced at 8^h 56^m A.M., temperature being 50°. The action was stopped at 12^h 45^m A.M., temperature being 98°. The action recommenced at 2 P.M., temperature 52°. Finally stopped at 4^h 41^m P.M., temperature being 97°.

Hence the total time was 6^h 30^m. The calculus when dried weighed 381 grains, so that the total loss was 22 grains, or about 3½ grains each hour. See fig. 8.

It appears then from this experiment that the effect on a uric acid calculus, when a dilute solution of nitre is employed, is less than one-half of the effect produced when a solution of nitre of double the strength is used.

From these experiments, it is evident that phosphatic and uric acid calculi can be dissolved in a very dilute solution of nitrate of potash, but the weaker the solution the longer the time required to obtain equal results.

Lastly, an experiment made on an undried phosphatic calculus, fresh from the bladder, showed that the action on the wet calculus was more rapid than when it was previously dried; but the nature of the experiment does not admit of a numerical statement of the result. See fig. 9. The electrodes in this experiment were much longer than those previously used.

The mechanical difficulties in making an instrument (litholyte) to be used in the body have not yet been completely overcome.

It is essentially requisite in such an instrument,—

First. That the insulation should be perfect, in order that the chemical action may be transferred to the surface of the stone.

Secondly. That no chemical action should be set up on the surface of the bladder or the urethra, whereby the mucous membrane might be injured.

Thirdly. It may be necessary that a double passage for the injection of the solution of nitre should exist, in order to keep down the temperature and to admit of the escape of the gas evolved in the bladder.

Already many instruments have been made for me by Mr. WEISS, yet hitherto a perfectly safe one has not enabled me to make experiments on the solution of calculi within the body.

EXPLANATION OF THE PLATE.

PLATE XIV.

Representing the exact size of the calculi and the extent of action.

Fig. 1 (a). Uric acid before it was acted on.

(b). The same after action.

Fig. 2. The electrodes of the exact size and shape used.

Fig. 3. Uric acid calculus acted on in the nitre solution.

Fig. 4. Phosphatic calculus in the same solution.

Fig. 5. Piece of marble in solution of nitre.

Fig. 6. Piece of marble in solution of Glauber-salt.

Fig. 7. Phosphatic calculus in a more dilute solution of nitre.

Fig. 8. Uric acid calculus in a more dilute solution of nitre.

Fig. 9. A calculus as moist as when taken from the bladder, acted upon in nitre with larger electrodes.

IX. *On Molecular Influences.*—PART I. *Transmission of Heat through Organic Structures.* By JOHN TYNDALL, F.R.S.

Received October 20, 1852.—Read January 6, 1853.

THE various solid substances which are met with in nature allow themselves to be classed under three general heads:—Amorphous, Crystalline and Organized. In amorphous bodies the component particles are confusedly mingled, without any regard to symmetry of arrangement. In crystalline bodies, on the contrary, the particles are symmetrically arranged; the mass appears as if built up according to certain architectural rules, and the result is an exterior form whose angular dimensions are perfectly constant for all crystals of the same class. Organized bodies, as the name implies, are bodies endowed with, or composed of, organs formed with reference to the special functions they are intended to discharge, and in the construction of which a molecular architecture of a very composite order comes into play. The granules, cells, glands, tubes, &c. of animal and vegetable tissues are all, of course, the visible products of this architecture. Crystalline bodies appear to bridge the chasm which separates the amorphous from the organized. Like the former, they are devoid of the powers of assimilation and reproduction—like the latter, their particles are arranged according to rule; as if nature, in the case of crystals, had made her first structural effort. The student of nature has ever looked upon these molecular combinations with an inquiring eye, and, perhaps, at no age of the world more than at present. The molecular peculiarities of any substance declare themselves by the manner in which a force is modified in its passage through the substance. The polarization and bifurcation of a luminous ray in doubly refracting media is an old example of molecular action; and the rotation of the plane of polarization, observed by Professor FARADAY, may be the result of a mechanical change of the medium, effected by the current or the magnet. SENARMONT'S* and KNOBLAUCH'S† experiments demonstrate the influence of crystalline structure upon the transmission of heat; and the magnecrystallic discoveries of PLÜCKER and FARADAY receive, I believe, their true explanation by reference, simply, to the modification of the magnetic and diamagnetic forces which peculiarity of aggregation induces. Matter, in this aspect, may be regarded as a kind of organ through which force addresses our senses; if the organ be changed, it is reasonable to infer that the utterance will be correspondingly modified,—an inference which is abundantly corroborated by experiment. Thus, mechanical pressure will polarize a ray, and the same may be

* *Annales de Chimie et de Physique*, vols. xxi. xxii. xxiii.

† *POGGENDORFF'S Annalen*, vol. lxxxv. p. 169.

applied with success to produce all the phenomena of magneocrystalline action. The anomalies which owe their origin to peculiarities of aggregation are indeed manifold, and constitute one of the most important subjects of study which can engage the attention of the natural philosopher.

Organic structures furnish an ample field for inquiries into molecular action. For here, as before remarked, nature, to attain her special ends, has arranged her materials in a particular manner. To ascertain what effect the molecular structure of wood has upon the transmission of heat through it, constitutes the object of the first part of this investigation.

Upwards of twenty years ago MM. DE LA RIVE and DECANDOLLE instituted an inquiry into the conductive power of wood*, and in the case of five specimens examined established the fact of the feeble conductivity of the substance, and also that the velocity of transmission was greater along the fibre than across it. The manner of experiment was that usually adopted in inquiries of this nature, and applied to metals by M. DEPRETZ†. A bar of the substance was taken, one end of which was brought into contact with a source of heat and allowed to remain so until a stationary temperature was assumed. The temperatures attained by the bar, at various distances from its heated end, were ascertained by means of thermometers fitting into cavities made to receive them; from these data, with the aid of a well-known formula, the conductivity of the wood was determined. Since the publication of their results by the distinguished men above mentioned, nothing, so far as I am aware of, has been done in connexion with this subject.

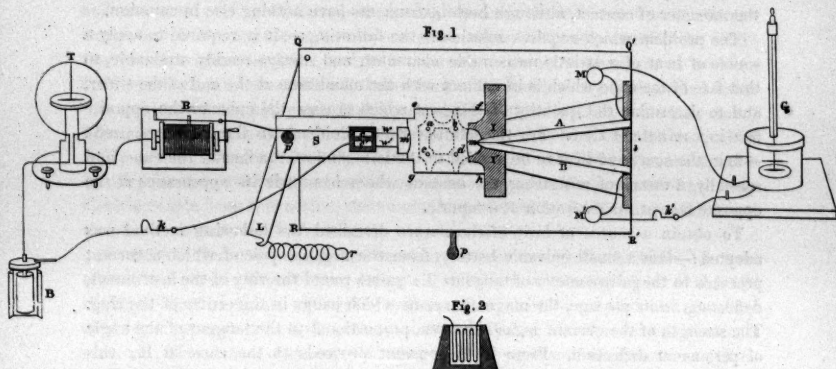
The mode of experiment here indicated is, however, by no means sufficiently delicate for an inquiry like the present. Some other mode must therefore be devised. I will not trouble the reader with a rehearsal of the long series of trials which have led to the construction of the instrument employed in these researches, but will proceed at once to the description of it.

QQ' RR', fig. 1, is an oblong piece of mahogany 3·4 inches long, 1·8 inch wide and 0·5 of an inch deep. A is a bar of antimony, B is a bar of bismuth, each measuring 1·5 inch in length, 0·07 of an inch in breadth and 0·3 of an inch in depth. The ends of the two bars are kept in close contact by the ivory jaws I, I', and the other ends are let into a second piece of ivory *i*, in which they are firmly fixed. Soldered to these ends are two pieces of platinum wire which proceed to the little ivory cups MM', enter through the sides of the cups and communicate with a drop of mercury placed in the interior. The wood is cut away, so that the bars A and B are sunk to a depth which places their upper surfaces a little below the general level of the slab of mahogany. The ivory jaws I, I' are sunk similarly. Two small projections are observed in the figure jutting from I, I'; across from one projection to the other a fine membrane is drawn, thus enclosing a little chamber *m*, in front of the wedge-like end of the bismuth and antimony junction; the chamber has an ivory bottom. S is a wooden

* Mém. de la Soc. de Genève, vol. iv. p. 70.

† Annales de Chim. et de Phys. December 1827.

slider, which can be moved smoothly back and forward along a bevelled groove, by means of the lever *L*. This lever turns on a pivot near *Q*, and fits into a horizontal slit in the slider, to which it is attached by the pin *p'* passing through both; in the lever an oblong aperture is cut through which *p'* passes, and in which it has a certain amount of lateral play, so as to enable it to push the slider forward in a straight line. A small chamber, *m'*, is cut out at the end of the slider, and across, from projection



to projection, a thin membrane is stretched; a chamber is thus formed bounded on three sides and the bottom by wood, and in front by the membrane. A thin platinum wire, bent up and down several times, so as to form a kind of micrometer grating, is laid against the back of the chamber and imbedded in the end of the slider by the stroke of a hammer; the end in which the wire is imbedded is then filed down until about half the latter is removed, and the whole is reduced to a uniform flat surface. Against the common surface of the slider and wire an extremely thin plate of mica is glued, sufficient, simply, to interrupt all contact between the bent wire and a quantity of mercury which the chamber *m'* is destined to contain: the ends *w w'* of the bent wire proceed to two small cisterns, *c c'*, hollowed out in a slab of ivory; they enter through the substance into the cisterns, and come thus into contact with mercury which fills the latter. The end of the slider and its bent wire are shown in fig. 2. The rectangular space *efgh*, fig. 1, is cut quite through the slab of mahogany, and a brass plate is screwed to the latter underneath; from this plate (which, for reasons to be explained presently, is cut away as shown by the dotted lines in the figure) four conical ivory points, *abcd*, project upwards; though appearing to be upon the same plane as the upper surfaces of the bismuth and antimony bars, the points are in reality 0.3 of an inch below the said surfaces.

The body to be examined is reduced to the shape of a cube, and is placed, by means of a pair of pliers, upon the four supports *abcd*; the slider *S* is then drawn up against the cube, and the latter becomes firmly clasped between the projections of the piece of ivory *II'*, on the one side, and those of the slider *S*, on the other. The chambers *mm'* being filled with mercury, the membrane in front of each is pressed gently against the cube by the interior fluid mass, and in this way perfect contact is secured. In fact the principle here applied is the same as that made use of by FOURIER* in his thermometer of contact, although both instruments have nothing else in common.

The problem which requires solution is the following:—It is required to apply a source of heat of a strictly measurable character, and always readily attainable, to that face of the cube which is in contact with the membrane at the end of the slider, and to determine the quantity of this heat which crosses the cube to the opposite face in a minute of time. For the solution of this problem two things are required:—first, the source of heat to be applied to the left hand of the face of the cube, and secondly, a means of measuring the amount which has made its appearance at the opposite face at the expiration of a minute.

To obtain a source of heat of the nature described the following method was adopted:—*B* is a small galvanic battery, from the negative pole of which a current proceeds to the galvanometer of tangents *T*; passes round the ring of the instrument, deflecting, in its passage, the magnetic needle which hangs in the centre of the ring. The strength of the current is, as is known, proportional to the tangent of the angle of permanent deflection. From *T* the current proceeds to the rheostat *R*; this instrument consists of a cylinder of serpentine stone, round which a German silver wire is coiled spirally; by turning the handle of the instrument any required quantity of this powerfully resisting wire is thrown into the circuit, the current being thus regulated at pleasure. The sole use of these two last instruments in the present series of experiments is to keep the current perfectly constant from day to day. From the rheostat the current proceeds to the cistern *c*, thence through the bent wire, and back to the cistern *c'*, from which it proceeds to the other pole of the battery.

The bent wire, during the passage of the current, becomes heated; this heat is transmitted through the mercury in the chamber *m'* to the membrane in front of the chamber; this membrane becomes the proximate source of heat which is applied to the left-hand face of the cube. The quantity transmitted from this source, through the mass of the cube, to the opposite face, in any given time, will, of course, depend on the conductivity of the latter, and its amount may be estimated from the deflection which it is able to produce upon the needle of a galvanometer connected with the bismuth and antimony pair. *G* is a galvanometer used for this purpose; from it proceed wires to the mercury cups *M M'*, which, as before remarked, are connected by platinum wires with *A* and *B*. The galvanometer is a carefully constructed and delicate instrument from the workshop of that skilful mechanic, KLEINER, in Berlin.

* Annales de Chimie et de Physique, March 1828.

The sole use of the mercury in the chambers m and m' is to secure good and equable contact; when the chambers are filled with pure mercury, and this is allowed to remain in them throughout an entire series of experiments, it is certain that the conditions of contact are perfectly constant, and thus the most fruitful source of doubt and error is effectually excluded. In rough experiments the chambers might be dispensed with, and the bent wire itself might be brought into contact with one face of the cube, while the other face might immediately press against the bismuth and antimony. The result however of many hundreds of experiments made with the instrument in this state, has been to prove the impossibility of preserving the conditions of experiment constant, and to compel me to devise some means of avoiding the irregularities which exhibited themselves. The instrument just described meets the requirements of the case; care is necessary in the use of it, but when care is taken, an accuracy is attainable by it which, I believe, has been hitherto unequalled.

The action of mercury upon bismuth, as a solvent, is well known; an amalgam is speedily formed where the two metals come into contact. To preserve our thermoelectric couple from this action, their ends are protected by a sheathing of the same membrane as that used in front of the chambers m m' .

Previous to the cube being placed between the two membranes, the latter, by virtue of the fluid masses behind them, bulge out a little, thus forming a pair of soft and slightly convex cushions. When the cube is placed upon its supports and the slider is brought up against it, both cushions are pressed flat, and thus perfect contact is secured. The surface of the cube is larger than the surface of the membrane in contact with it*; and thus the former is always firmly caught between the opposed rigid projections, the slider being held fast in this position by means of the spring r , which is then attached to the pin p . The exact manner of experiment is as follows:—Having first seen that the needle of the galvanometer points to zero, when the thermocircuit is complete, the latter is interrupted by means of the break-circuit key K . At a certain moment, marked by the seconds-hand of a watch, the voltaic circuit is closed by the key k , and the current is permitted to circulate for sixty seconds; at the sixtieth second the voltaic circuit is broken by the left hand at k , while almost at the same instant the thermo-circuit is closed by the right hand at K . The needle of the galvanometer is instantly deflected, and the limit of the first impulsion is noted; the amount of this impulsion depends, of course, upon the quantity of heat which has reached the bismuth and antimony junction through the mass of the cube during the time of action, and consequently upon the conductive power of the latter. The limit of the first impulsion being noted, the cube is instantly removed, and the instrument is allowed to cool until the needle of the galvanometer returns to zero. To expedite the cooling, the metallic surfaces of A and B are to a great extent exposed; the wood is cut away all round them, and from the space between them; they do not rest upon the wood, their sole points of support being the ivory i at one end and the

* The edge of each cube measured 0.3 of an inch.

jaws II' at the other. The cube, as before explained, does not touch the brass plate underneath it, but is supported on its four conical points, and the plate which bears these is itself as much as possible cut away to permit of a free circulation of air through the space *efgh*. Time is a precious commodity to the experimenter, and by the means described the cooling is hastened and the experiments can succeed each other more quickly. To hasten the cooling further I made use of a pair of small bellows during the first minute after the removal of each cube, and, afterwards, a plate of thin glass was placed over the junction, but not in contact with it. On the glass two drops of ether were suffered to fall from a pipette; its evaporation caused a refrigeration of the air underneath, which, in virtue of its increased density, sank and diffused itself around the place of junction. In this way the temperature at the junction was brought a little lower than that of the surrounding air; the needle of the galvanometer being thus brought back, not only to zero, but to a certain point at the other side of it; at this point the glass was removed and a new cube was introduced; the thermo-circuit was permitted to remain closed until the needle descended to zero, which it slowly did, when the cause of local cooling was removed; the thermo-circuit was then broken at *K*, and things stood as at the commencement of the former experiment. The voltaic circuit was once more closed, the current permitted to circulate sixty seconds, then interrupted by the left hand, the thermo-circuit being closed at the same moment with the right, and the first impulsion measured as before.

When however these artificial means of cooling are adopted great care is necessary. We must not use the bellows in some experiments and neglect the use of it in others; and if the ether be applied once, it must be applied throughout the entire series of experiments. It must continue to act for the same time, and the same quantity should be applied in all cases. Of course such precautions are only necessary when great accuracy is required, but here they are absolutely necessary. Judging from the description, the mode of experiment may appear complicated, but in reality it is not so. A single experimenter has the most complete command over the entire arrangement. The wires from the small galvanic battery (four of BUNSEN's cells) remain undisturbed from day to day; all that is to be done is to connect the battery with them, and every thing is ready for experiment.

There are in wood three lines at right angles to each other, which the mere inspection of the substance enables us to fix upon as the necessary resultants of molecular action: the first line is parallel to the fibre; the second is perpendicular to the fibre, and to the ligneous layers which indicate the annual growth of the tree; while the third is perpendicular to the fibre and parallel, or rather tangent to the layers. From each of a number of trees a cube was cut, so that every two opposite faces were parallel to one of the above lines. Thus, two faces were parallel to the ligneous layers, two perpendicular to them, while the remaining two were perpendicular to the fibre. It was proposed to examine the velocity of calorific transmission through the

mass in these three directions. It may be remarked that the cubes were fair average specimens of the woods, and were in all cases well-seasoned and dry.

The cube was first placed upon its four supports *abcd*, so that the line of flux from *m'* to *m* was parallel to the fibre, and the deflection produced by the heat transmitted in sixty seconds was observed. The position of the cube was then changed so that its fibre stood vertical, the line of flux from *m'* to *m* being perpendicular to the fibre and parallel to the ligneous layers; the deflection produced by a minute's action in this case was also determined. Finally, the cube was turned 90° round, its fibre being still vertical, so that the line of flux was perpendicular to both fibre and layers, and the consequent deflection was observed. In the comparison of these two latter directions the chief delicacy of manipulation is necessary. It requires but a rough experiment to demonstrate the superior velocity of propagation along the fibre, but the velocities in all directions perpendicular to the fibre are so nearly equal that it is only by the greatest care and, in the majority of cases, by numerous experiments, that a differential action can be securely established.

The following table contains the results of the inquiry; it will explain itself.

TABLE I.—Strength of Current used to heat the bent Wire, as measured by the tangent galvanometer:—constantly 35°. Deflections, the line of flux being of:—

Description of wood.	I. Parallel to fibre of wood.	II. Perpendicular to fibre and parallel to lig- neous layers.	III. Perpendicular to fibre and to ligneous layers.	Description of wood.	I. Parallel to fibre of wood.	II. Perpendicular to fibre and parallel to lig- neous layers.	III. Perpendicular to fibre and to ligneous layers.
American Birch.....	35	5	11			10.5	11.0
Oak.....	33	9.5	11			9.5	11.0
						10.5	9.5
						8.0	10.5
		8.0	11.0	Black Ebony	32	Mean 9.5	Mean 10.5
		9.0	10.0				
		9.7	11.2	Bird's-eye Maple ...	31	11	12
		9.5	11.0				
Beech	33	Mean 8.8	Mean 10.8			9.8	12.0
						10.0	11.0
		10.2	12.2			11.5	12.0
		10.2	12.0			11.0	13.5
		9.5	12.0	Lance-wood	31	Mean 10.6	Mean 12.1
Coromandel-wood. An exceedingly hard wood from Ceylon	33	Mean 9.8	Mean 12.3				
						8.0	10.0
Quebec Pine	33	10	11	Zebra-wood.		8.0	10.0
				The produce of the		8.5	10.0
		9.8	11.0	Brazils	31	Mean 8.2	Mean 10.0
		10.0	12.0				
		10.5	11.5			9.8	12.3
Beef-wood. A red-coloured wood from New South Wales	33	Mean 10.0	Mean 11.4			10.0	11.7
						10.0	12.0
						10.0	12.0
				Box-wood	31	Mean 9.9	Mean 12.0

TABLE I. (Continued.)

Description of wood.	I. Parallel to fibre of wood.	II. Perpendicular to fibre and parallel to li- gneous layers.	III. Perpendicular to fibre and to ligneous layers.	Description of wood.	I. Parallel to fibre of wood.	II. Perpendicular to fibre and parallel to li- gneous layers.	III. Perpendicular to fibre and to ligneous layers.
		11.5 11.5 10.5 11.0	12.5 12.0 12.0 12.0	King-wood. Called also violet- wood, from the Bra- zils	30	10.0 10.5 10.5	12.0 12.0 11.0
Tamarind-wood	31	Mean 11.1	Mean 12.1	Bladder-nut-wood ...	29	10.0	12.0
		10.0 9.5 10.0 10.0	12.0 12.0 13.0 11.5	Larch	29	10.0	11.0
Teak-wood	31	Mean 9.9	Mean 12.4			11.0 10.0 11.2 12.0	14.0 12.0 14.0 12.0
		10.5 10.0 10.0 11.0	12.0 13.5 13.0 12.0	Princes-wood. From Jamaica	29	Mean 11.1	Mean 13.1
Rose-wood	31	Mean 10.4	Mean 12.6			11.5 11.5 12.5 11.2	13.0 12.0 12.8 12.5
		10.5 10.5	13.0 12.0	Green Heart. From Jamaica	29	Mean 11.4	Mean 12.6
Mazatlan-wood	30	Mean 10.5	Mean 12.5	Walnut	28	11.0	13.0
		12.5 11.0 12.0 12.0	12.5 12.0 12.7 12.0	Drooping Ash	28	11.0	12.0
Satin-wood. From St. Domingo...	30	Mean 11.9	Mean 12.3	Botany Bay Oak. Does not belong to the same genus as the European.....	28	Mean 9.9	Mean 12.4
		9.0 9.0 10.0 9.0	11.0 10.5 11.0 11.5			12.0 12.5 12.0 11.2	13.0 14.0 13.5 14.0
Brazilletto. From Jamaica	30	Mean 9.2	Mean 11.0	Cocoa-wood. From the West In- dies	28	Mean 11.9	Mean 13.6
		10.0 10.0 10.0	10.5 11.5 11.0			11.3 11.2 10.5 10.0	12.0 12.5 11.8 11.0
Locust-wood. From North America.	30	Mean 10.0	Mean 11.0	Madagascar Red- wood	28	Mean 10.7	Mean 11.8
		11.0 10.0 10.0	11.0 11.0 11.5			11.0 10.5 9.5 9.0	12.0 12.5 11.5 11.0
Ruby-wood. From Calcutta	30	Mean 10.3	Mean 11.2	Sandal-wood. From Malabar	28	Mean 10.0	Mean 11.7
		11.0 10.5 10.5	12.0 12.0 11.0				
Peruvian-wood	30	Mean 10.7	Mean 11.7				

TABLE I. (Continued.)

Description of wood.	I. Parallel to fibre of wood.	II. Perpendicular to fibre and parallel to li- gneous layers.	III. Perpendicular to fibre and to ligneous layers.	Description of wood.	I. Parallel to fibre of wood.	II. Perpendicular to fibre and parallel to li- gneous layers.	III. Perpendicular to fibre and to ligneous layers.
Tulip-wood. From Brazil	28	Mean 11.0	Mean 12.1	Iron-wood. Sp. gr. 1.426	26	Mean 10.2	Mean 12.4
		11.0 11.0 11.0 11.0	11.5 12.5 12.5 12.0			9.5 9.5 10.2 10.5	12.5 12.0 13.0 12.0
Campbor-wood. From China	28	Mean 8.6	Mean 10.0	Chestnut.....	26	Mean 10.1	Mean 11.5
		9.0 9.0 8.0	10.0 10.0 10.0			10.2 10.0	12.0 11.0
Olive-wood. From Leghorn	28	Mean 10.5	Mean 13.2	Sycamore	26	Mean 10.6	Mean 12.2
		10.0 11.0 10.5	14.0 12.0 13.0			10.2 11.0	12.5 12.0
Gaffie Deal.....	27	10	11	Spruce Fir	25	11.8	12.5
Ash.....	27	9.5	11.5	Honduras Mahogany	25	9.0	10.0
		11.0 11.0 9.0 11.0	12.5 12.0 13.0 11.5	Brazil-wood. A red dye-wood, called also Per- nambuco.....	25	Mean 11.9	Mean 13.9
Green Ebony. From Jamaica	27	Mean 10.5	Mean 12.2	Yew	24	11.0	12.0
		8.0 7.0 8.0 8.0	9.0 10.0 10.0 9.0	Elm.....	24	10.0	11.5
Black Oak	27	Mean 8.0	Mean 9.4	Plane-tree	24	10.0	12.0
Apple-tree	26	10	12.5	Portugal Laurel.....	24	10.0	11.5
		13.0 13.0 14.0 13.5	15.0 15.0 15.0 15.0	Bullet-wood. From the Virgin Isles, West Indies	24	Mean 10.0	Mean 11.7
Cam-wood. An African dye-wood	26	Mean 13.4	Mean 15.0	Spanish Mahogany	23	11.5	12.5
				Scotch Fir	22	10.0	12.0
				Laurel.....	22	12.0	15.0

To enable the eye to detect, at once, the law of action established by the experiments, we will present the results in a more condensed form.

Deflections.

Description of wood.	I. Parallel to fibre.	II. Perpendi- cular to fibre and parallel to ligneous layers.	III. Perpendi- cular to fibre and to ligneous layers.	Description of wood.	I. Parallel to fibre.	II. Perpendi- cular to fibre and parallel to ligneous layers.	III. Perpendi- cular to fibre and to ligneous layers.
1 American Birch	35	9.0	11.0	28 Botany Bay Oak	28	9.9	12.4
2 Oak	34	9.5	11.0	29 Cocoa-wood	28	11.9	13.6
3 Beech	33	8.8	10.8	30 Madagascar Red-wood	28	10.7	11.3
4 Coromandel-wood.....	33	9.8	12.3	31 Sandal-wood	28	10.0	11.7
5 Quebec Pine	33	10.0	11.0	32 Tulip-wood	28	11.0	12.1
6 Beef-wood	33	10.0	11.4	33 Camphor-wood	28	8.6	10.0
7 Black Ebony	32	9.5	10.5	34 Olive-tree	28	10.5	13.2
8 Bird's-Eye Maple	31	11.0	12.0	35 Gaffle-Denl	27	10.0	11.0
9 Lance-wood	31	10.6	12.1	36 Ash	27	9.5	11.5
10 Zebra-wood	31	8.2	10.0	37 Green Ebony	27	10.5	12.2
11 Box-wood	31	9.9	12.0	38 Black Oak	27	8.0	9.4
12 Tamarind-wood.....	31	11.1	12.1	39 Apple-tree	26	10.0	12.5
13 Teak-wood	31	9.9	12.4	40 Cam-wood	26	13.4	15.0
14 Rose-wood.....	31	10.4	12.6	41 Iron-wood	26	10.2	12.4
15 Mazatlan-wood	30	10.5	12.5	42 Chestnut	26	10.1	11.5
16 Satin-wood	30	11.9	12.3	43 Sycamore	26	10.6	12.2
17 Braziletto	30	9.2	11.0	44 Spruce Fir.....	25	11.8	12.5
18 Locust-wood	30	10.0	11.0	45 Honduras Mahogany	25	9.0	10.0
19 Ruby-wood	30	10.3	11.2	46 Brazil-wood	25	11.9	13.9
20 Peruvian-wood	30	10.7	11.7	47 Yew	24	11.0	12.0
21 King-wood	30	10.3	11.7	48 Elm	24	10.0	11.5
22 Bladder-nut-wood.....	29	10.0	12.0	49 Plane-tree	24	10.0	12.0
23 Larch.....	29	10.0	11.0	50 Portugal Laurel	24	10.0	11.5
24 Princes-wood.....	29	11.1	13.1	51 Bullet-wood	24	10.0	11.7
25 Green-heart	29	11.4	12.6	52 Spanish Mahogany	23	11.5	12.5
26 Walnut	28	11.0	13.0	53 Scotch Fir.....	22	10.0	12.0
27 Drooping Ash	28	11.0	12.0	54 Laurel	22	12.0	15.0

The above table furnishes us with the fullest corroboration of the result arrived at by DE LA RIVE and DE CANDOLLE, regarding the superior conductivity of the wood in the direction of the fibre. Evidence is also afforded as to how little mere density affects the velocity of transmission. There appears to be neither law nor general rule here. American Birch, a comparatively light wood, possesses undoubtedly a higher transmissive power than any other in the list—a result which has been established by numerous experiments, although but one appears cited in the table. Iron-wood, on the contrary, with a specific gravity of 1.426, stands low. Again, Oak and Coromandel-wood,—the latter so hard and dense that it is used for sharp war-instruments by savage tribes,—stand near the head of the list, while Scotch Fir and other light woods stand low.

We further find that the lateral conductivity bears no definite ratio to the longitudinal conductivity. Indeed the tendency appears to be that those woods which possess the lowest power of transmission, along the fibre, possess the highest power

across it. But here the exceptions are so numerous that we have no warranty for a general conclusion.

But the most remarkable result of the experiments remains yet to be stated. If we cast our eyes along the second and third columns of the tabular summary, we shall find that in every instance the velocity of propagation is greatest in a direction perpendicular to the ligneous layers. This result is, of course, wholly independent of the specific heat of the wood, inasmuch as it is two directions through the self-same cube which are here compared with each other. The law of molecular action, as regards the transmission of heat through wood, may therefore be expressed as follows:—

At all the points not situate in the centre of the tree, wood possesses three unequal axes of calorific conduction, which are at right angles to each other. The first, and principal axis, is parallel to the fibre of the wood; the second, and intermediate axis, is perpendicular to the fibre and to the ligneous layers; while the third and least axis is perpendicular to the fibre and parallel to the layers.

The researches of SAVART on the sonorous vibrations of wood naturally suggest themselves here; for, doubtless, the same molecular structure which imparts to this substance the peculiar elastic properties discovered by SAVART, must be regarded as the cause of the differential action established above. SAVART* took bars of equal size, and in different directions, from a mass of wood; determining their resistance to flexure from the number of vibrations carried out by each in a certain time, he found that wood possessed three axes of elasticity. *These axes coincide with the axes of calorific conduction established by the foregoing experiments.* The axis of greatest elasticity coincides with that of highest conductive capacity, and the axis of least elasticity with that of lowest conductive capacity.

A few exact experiments made with a view to ascertain the influence of molecular structure upon the cleavage of wood would have formed an interesting addition to this communication; fortunately, however, the mere sense of touch, to fingers accustomed to seek for the cleavages of crystals, affords sufficient evidence here. If a piece of wood be taken, on which the rings which mark the growth of the tree plainly appear, and a penknife or a chisel be laid across the rings, it will be found that the pressure necessary to cleave the wood is less in this than in any other direction. The cohesion in the direction parallel to the layers and perpendicular to the fibre is therefore a minimum. In the same way, it will be found that of all lines perpendicular to the fibre the line of greatest cohesion is perpendicular to the ligneous layers; while the cohesion in the direction of the fibre is far greater than along either of the lines just referred to. Hence—

Wood possesses three axes of cohesion which coincide with the axes of calorific conduction—the greatest with the greatest, and the least with the least.

It would have also added interest to the inquiry to have examined the permeability

* TAYLOR'S Scientific Memoirs, vol. i. p. 139.

of wood to fluids in various directions. Here, again, however, the experimental knowledge already amassed by housewives and cask-makers comes to our aid. It is well known that fluids would ooze with facility through a stave cut perpendicular to the fibre; a wooden plate, for instance, cut perpendicular to the axis of a tree would be totally unfit for the bottom of a vessel destined to hold spirits, water, or brine. Further precautions, however, must be taken in choosing staves for casks. If the surface of the stave be parallel to the ligneous layers, the liquid, though with greater difficulty than in the case just mentioned, will still make its way through. The stave must be cut perpendicular to the layers; for, in crossing such a stave, the resistance offered to the passage of the fluid is a maximum. Hence—

Wood possesses three axes of fluid permeability which coincide with those of calorific conduction,—the greatest with the greatest, and the least with the least.

To sum up:—In this single substance we have pointed out the existence of three new systems of axis; the axes of calorific conduction, of cohesion, and of fluid permeability; all of which coincide with a fourth system of axes of elasticity discovered by SAVART. The experiments also furnish an illustration of the theory of Professor STOKES, who proves that the flux of heat through any body may be referred to three rectangular axes, which he calls the thermic axes of the body*.

MM. DE LA RIVE and DECANDOLLE have remarked upon the influence which its feeble conducting power in a lateral direction must exert in preserving within a tree the warmth which it acquires from the soil. In virtue of this property a tree is able to resist sudden changes of temperature which would probably be prejudicial to it; it resists alike the sudden abstraction of heat from within and the sudden accession of it from without. But nature has gone further, and clothes the tree with a sheathing of worse-conducting material than the wood itself, even in its worst direction. The following are the deflections obtained by submitting a number of cubes of bark of the same size as the cubes of wood to the same conditions of experiment:—

	Deflection.	Corresponding deflection produced by the wood.
Beech-tree Bark	7	10·8
Oak-tree Bark	7	11·0
Elm-tree Bark	7	11·5
Pine-tree Bark	7	12·0

The direction of transmission, in these cases, was from the interior surface of the bark outwards.

The average deflection produced by a cube of wood, when the flux is lateral, may be taken at

12°;

* Cambridge and Dublin Mathematical Journal, November 1851.

a cube of rock-crystal (pure silica) of the same size produces a deflection of 90°.

This single experiment is sufficient to show how different must be the meteorological effects of these two substances, when they exist in sufficient quantity to exercise an influence upon climate. Among the more prominent influences here, HUMBOLDT mentions the nature of the soil and of vegetation. The general influence of an arid and exposed soil has been long known, but the part played by this substance, silica, has hitherto had no particular importance attached to it. Were gypsum, however, instead of silica, the prevalent mineral in Sahara, a very different state of things from the present would assuredly exist. A cube of the latter substance examined in the usual manner produces a deflection of

19°

only. It is scarcely superior to wood, while there is the strongest experimental grounds for the belief that silica possesses a higher conductive power than some of the metals. These grounds shall be adduced in a future paper.

Let us consider, for a moment, the process which takes place from sunrise to the hour of maximum temperature in a region overspread with forests, and compare it with that which must take place in the African Desert. In the former case, the heat slowly and with difficulty penetrates the masses of wood and leaves on which it falls, and after the point of maximum temperature is passed, the yielding up of the heat acquired is proportionately slow. In the desert, however, the mass of silica exposed to the sun becomes burning hot as the hour of maximum temperature approaches; but, after this is passed, the heat is yielded up with proportionate facility. Hence a maximum and minimum thermometer must, in the latter case, mark a far wider range of temperature during the twenty-four hours than in the former. This agrees with observation. In Sahara, to use the words of Mrs. SOMERVILLE, during "the glare of noon the air quivers with the heat reflected from the red sand, and in the night it is chilled under a clear sky sparkling with its host of stars*." Were gypsum, however, the prevailing mineral, it is *à priori* certain that this could not be the case to anything like its present extent.

The following experiments furnish some notion of the transmissive power of a few other organic structures: cubes of the substances were examined in the usual manner.

Tooth of Walrus	16
Tusk of East Indian Elephant	17
Whalebone	9
Rhinoceros's-horn	9
Cow's-horn	9

Considering the density and elasticity of ivory, we might be disposed to attribute to it a comparatively high conductive power; but the experiment proves it to be a very

* Physical Geography, vol. i. p. 147.

bad conductor—much inferior, indeed, to wood in the direction of the fibre. Doubtless this conduces to the animal's comfort. Exposed to the rays of a tropical sun, if these huge bony masses were capable of assuming a high temperature during the day and losing it again at night, it must be a source of the greatest inconvenience to the animal, as at present constituted. The horns of the Rhinoceros and Cow, however, still more strikingly exemplify that fitness of parts which is perpetually presented to the student of natural science. In the latter case especially, the mass of horn in close contact with the skull, and therefore capable of transmitting heat directly to the animal's brain, must be attended with very unpleasant consequences, if horn were a good conductor. Given such a constitution, the substance fixed upon by our own enlightened intellect to furnish the animal with such weapons of defence, would be just such as nature has chosen.

As a general rule, sudden changes of temperature are prejudicial to animal and vegetable health; the substances used in the construction of organic tissues are exactly such as are best calculated to resist those changes. Coal enters largely into the composition of such tissues, and it is an exceedingly bad conductor. Here are the deflections obtained with three different descriptions of this substance:—

Sunderland coal	8
Boghead cannel	8
Lesmahago cannel	8

The following results illustrate the subject in a still more striking manner. It is almost needless to remark that each of the substances mentioned was reduced to the cubical form, and submitted to an examination similar in every respect to that of wood and quartz. While, however, a cube of the latter substance produces a deflection of 90°, a cube of

Sealing-wax produces a deflection of . . .	0
Sole leather	0
Bees'-wax	0
Glue produces a deflection of	0
Gutta-percha	0
India-rubber	0
Filbert-kernel	0
Almond-kernel	0
Boiled ham-muscle	0
Raw veal-muscle	0

The substances here named are all of them animal and vegetable productions; and the experiments demonstrate the extreme imperviousness of every one of them. Starting from the principle that sudden accessions or deprivations of heat are prejudicial to animal and vegetable health, we see that the materials chosen are precisely

those which are best calculated to avert such changes. It is yet to be estimated what influence the extreme non-conductibility of muscular tissue exerts in producing the remarkable constancy of temperature observed in the human body in different climates. The cuticle is an exceedingly bad conductor, and this explains the insensibility to heat of hands on which the skin has been thickened by exposure. Probably many escapes from the fiery ordeal, which have been hitherto referred to collusion, might be scientifically explained by reference to this fact. While studying at Marburg, I have sometimes heard Professor BUNSEN make a good-humoured remark on the tenderness of his pupils' fingers. Accustomed as he was to the manipulation of the glass used in his admirable eudiometrical researches, his fingers had acquired an insensibility to heat sufficient to carry him safely through an ordeal which, in other cases, would undoubtedly invoke the judicial condemnation of the middle ages. The experiments of CHANTREY and BLAGDEN are often referred to as illustrations of the surprisingly high temperature to which the human body may for a short time be exposed without injury. These experimenters owed their safety to two things,—to the non-conductibility of their tissues, and the non-conductibility of the air in contact with them. Were either of these materials changed, the experiments could not have been made. If air were a good conductor, and parted with its heat readily, their hands and faces would have shared the fate of the beefsteak and eggs which were cooked in contact with tin in the same oven. Were their bodies good conductors, they would have become heated like the tin, the heat would have been transferred to the deeper tissues and organs, to the probable destruction of the latter. As it was, however, both the causes mentioned contributed to the success of the experiment, and a mere surface irritation was the only inconvenience felt.

X. *On the Impregnation of the Ovum in the Amphibia. (Second Series, Revised.)*
And on the Direct Agency of the Spermatozoon. By GEORGE NEWPORT, F.R.S.,
 F.L.S. &c.

Received May 10,—Read June 17, 1852.

HAVING shown in a former series of investigations, which has been honoured by a place in the Philosophical Transactions for 1851, that the sole agent of impregnation of the ovum, in all cases of communion of the sexes, is the spermatozoon,—and having then supplied both direct and negative evidence that impregnation is not effected by the *liquor seminis*,—I endeavoured, in a subsequent communication to the Royal Society, in June 1851*, to arrive at some knowledge of the manner in which impregnation is effected, and of the nature of the impregnating influence.

Up to that period, and indeed, until very recently, I had never been able to detect any evidence of the existence of spermatozoa within the envelopes of the fecundated egg, but had constantly found them in great abundance, and easily recognized, in contact with the exterior surface. Experiment also, made by immersion of the egg in coloured fluids, showed that the substance of the envelopes, although permeable by fluids, is uniform in its structure; but no evidence was afforded by it of any natural canal, fissure, or perforation through the envelopes of the egg of the Frog, capable of admitting the spermatozoon to the interior, as has been supposed to exist in the egg of the Mammalia†.

Hence the conclusion which seemed to be fairly led to, was, that some influence was transmitted from the spermatozoon on the surface of the envelopes, through their substance, to the yolk which they inclose,—as the *commencement* of impregnation. But it was especially pointed out in my First Series‡, that “simple contact of the spermatozoon does not appear to be sufficient to determine the transmission of more or less of the material structural characters of the male parent to the offspring;” and that, “possibly, we may hereafter find that the first changes induced by contact of the impregnating body are completed by its diffuence, and by the material constituents into which it is dissolved, being transferred to the yolk by endosmosis;” Further, in my second communication, I remarked,—“I am not yet prepared to assent to the view, that simple contact alone, even of the spermatozoon, is sufficient to complete the changes which result in the formation of the embryo§.” Again, in

* Proceedings of Royal Society, vol. vi. p. 82.

† Philosophical Transactions, 1840, p. 533, Plate XXII. figs. 165, 167.

‡ Philosophical Transactions, 1851, p. 242.

§ See “Impregnation of the Ovum (Second Series),” June 19th, 1851, MS. No. 762, p. 49, in the Archives of the Royal Society.

a subsequent part of the same communication, these considerations are further referred to, as being those alone on which the transmission of structural peculiarities seemed likely to be explained*.

Recent observation has now supplied to me a fact which renders these considerations, which heretofore were regarded as hypothetical, much more probable; but, at the same time, it necessitates a revision of the view that an impregnating influence is transmitted from the surface to the interior of the egg; and also some correction of the deductions from experiment by which this view seemed to be supported.

The fact referred to, is, that the spermatozoon of the Frog penetrates bodily into the substance of the thick envelopes of the egg, and comes into direct communication with, at least, the membrane which incloses the yelk in the interior.

It is my duty, therefore, to Science, and to the Royal Society, knowing this to be the case, to revise and to extend my views, and to submit them again to the Society in their amended form; making, in self-correction, the certainty of fact now take the place of negative observation.

In doing this, I shall endeavour to show, that, although still regarding the spermatozoon, for reasons to be adduced, as the organ of a special condition of force, or vitality in the male body,—its influence on the egg in fecundation is direct and immediate, and not operative, as heretofore supposed, merely through the envelopes; but, nevertheless, as I have before shown, that it is exerted only so long as the spermatozoon continues to give evidence of its vitality or force in its power of motion.

Before proceeding to show that the spermatozoon penetrates into the envelopes of the egg it is necessary that I should point out some of the conditions which affect the fulfilment of its fecundatory function, both in regard to the spermatozoon itself and to the ovum. These are, on the one hand, the persistence of vital power in the spermatozoon as influenced by the temperature of the surrounding medium; and on the other, the endosmic property of the gelatinous coverings of the egg, and the susceptibility of the yelk which they inclose, to become impregnated.

1. THE VITALITY OF THE SPERMATOZOON AND OVUM COMPARED.

1. *Of the Spermatozoon.*—It has been elsewhere shown† that when the mature spermatic fluid of the Frog has been more than four hours removed from the living body, and mixed with water, at a temperature of 50° FAHR. or but a few degrees higher, it usually has already ceased to have any power to fecundate the egg; and its efficiency has been found to have been diminished in proportion to the length of time it has been obtained and so mixed. Nearly the whole of the observations I have since made have coincided with these results. Yet there have been two marked instances in which fecundation was effected by fluid, which had been obtained, and

* *Loc. cit.* p. 56.

† *Philosophical Transactions*, 1851, p. 218.

mixed with a small quantity of water, *twenty-four hours* before it was employed. In the first case, the fluid was obtained when the temperature of the atmosphere was 51° FAHR., and afterwards sunk to 49° FAHR., but had again risen to 51° FAHR. at the time of the experiment. When examined with the microscope, immediately before it was used, the fluid still contained a quantity of very active vibratile spermatozoa, and also an abundance of spermatozoal cells, still in course of development. This fact seemed to afford an explanation of the cause of the efficiency of the fluid for so great a length of time, spermatozoa being liberated from the cells during the whole period. Thus the fact of the prolonged fecundity of this fluid seems to confirm, instead of oppose the conclusion arrived at,—that, the more mature the fluid is before its removal from the body, the more efficacious it is, but only for a given extent of time, since it is then composed almost entirely of very active spermatozoa, with but very few cells of development. SPALLANZANI, as formerly mentioned*, found that the fluid of the foetid terrestrial Toad (*Bufo calamita*?) at a high temperature of the season, 70° FAHR. to 73° FAHR., at which this species spawns in Italy, had lost its fecundatory influence at the end of six hours†; but that in the temperature of an ice-house, 40° FAHR. it retained its efficacy for twenty-five hours‡. Further, that when the fluid was preserved in the testes of the dead animal, at the temperature of the season mentioned, it became effete in nine or ten hours; but that it continued to be efficacious, when retained in those organs in the temperature of an ice-house for thirty-four hours. Again, he found that the fluid of the green aquatic Frog (*Rana esculenta*), when mixed with a large quantity of water, and placed in an ice-house, the temperature of which he gives at 3½ REAUM. (39·87° FAHR.) retained its fecundatory property for thirty-five hours§. But it must be borne in mind, with regard to these observations, that the species of Toad (*Bufo*), pair later in the season, and at a higher temperature than those of the Frog; and also that the fluid employed by SPALLANZANI in all his experiments, was obtained by vivisection, either from the testes or the vesicles; and no doubt contained, as in my experiment just mentioned, many undeveloped cells; and, consequently, not being fully matured, retained its influence longer than it would have done, under the same circumstances, in its perfect condition.

BUT PREVOST and DUMAS||, in some experiments made to ascertain the length of time during which the fluid of the species they experimented on,—the *Rana esculenta*,—retains its fecundatory property, observed that it was efficient at *twenty-four hours*, although removed from the body and preserved during that time, in a temperature that varied from 18 to 22 cents., or from 64° to 71° FAHR.¶ This result, differing so much, in regard to temperature, from the results formerly arrived at by SPALLANZANI, in regard to the Frog, and from the majority of those since obtained by myself, may, I think, in great part be accounted for in the fact that, like the Toads, the *Rana*

* Loc. cit. p. 212.

† Dissertations, &c., vol. ii. p. 169.

‡ Loc. cit. p. 169.

§ Loc. cit. p. 194.

|| Annales des Sc. Naturelles, tom. ii. 1824.

¶ Loc. cit. p. 140.

esculenta spawns at a higher temperature, and later in the season than the *R. temporaria*, the subject of my observations, and that the fluid employed by PREVOST and DUMAS, was always obtained, as they state, by vivisection, and was expressed from the testes, and necessarily must have been in great part immature, and contained many spermatozoal cells, from which the fecundatory agents were liberated at different periods during the observations, and thus appeared to show that the fluid continues to be efficacious, at a high temperature of the atmosphere, for a much longer period than is really the case. In my own experiments the fluid employed being obtained, without vivisection, by simple compression of the body from the seminal ducts and vesicles, has usually contained only perfectly mature and very active spermatozoa, with but few cells of development. In this condition, as I have stated, it has retained its fecundatory property only during the first three or four hours. Yet, in the exception above mentioned, and also in the second one alluded to, which occurred more recently, the fluid was efficacious at the end of *twenty-four hours* in the first, and *twenty-six hours* in the second instance, the temperature being similar on the two occasions. In both instances the fluid contained a very large quantity of developmental cells, and had been procured from individuals which were not fully prepared for the exercise of their fecundatory function. Further, I may add, that in each of these instances, after the fluid had been supplied to some eggs to test its effect, I found many undeveloped spermatozoal cells adhering to the surface of the eggs, and detected some spermiatic bodies in the act of being liberated from them. These facts, then, are confirmatory of the suggestion respecting the results obtained by PREVOST and DUMAS.

The presence of undeveloped cells in the fluid obtained from the living Frog is explicable in two or three ways: first, in that of the animal having but recently been taken from its natural haunts, either early in the season, or at a later period, after a continuance of unusually cold weather and easterly wind, or after the sexes have but recently united; in either of which cases, as also in certain others, in which the individual itself has been late in the development of its reproductive organism, developmental cells are thrown off from the testes together with already liberated spermatozoa, and are usually abundant in the efferential ducts and vesicles. It may thus be seen that in judging of the length of time during which the fluid preserves its fecundatory influence, and consequently, the spermatozoon its vitality, after removal from the body, it is necessary to take into consideration, not only the temperature of the surrounding medium, but also the precise condition of the fluid itself. In perfectly natural impregnation by the Frog there is reason to believe that the fluid contains but very few undeveloped cells, and that it is not employed until it has acquired its full maturity, and that,—as the time of its actual employment is exceedingly brief,—the oviposition of *Rana temporaria* occupying scarcely a single minute, the retention of the impregnating influence and vital force of the spermatozoon may be even for a much shorter space of time than I have mentioned—from three to four

hours. But this time may differ in different species of the *Anoura*, and of the higher vertebrata, and may have some relation to the particular economy of each animal.

The general conclusion which seems to be deducible from a comparison of the observations of SPALLANZANI and of PREVOST and DUMAS, with those by myself, in regard to the tail-less Amphibia, is, that—making allowance for the difference of species and habit of the animals experimented on, and for the degree of maturity of the fluid, which there seems reason to think does not attain its perfect condition until it has passed into the efferential ducts and vesicles—the vitality of the spermatozoon, and the duration of its fecundatory power, are in a ratio *inverse* to that of an increase of temperature in the surrounding medium.

2. EVOLUTION OF VITALITY IN THE SPERMATOZOON AND OVUM.

WAGNER and LEUCKARDT have already pointed out* that the power of motion possessed by the spermatozoon is closely connected with the completion of its structure and composition, and is gradually evolved as the development of this body proceeds. This power of motion I shall presently endeavour to show I regard as the visible exponent of its fecundatory force, or form of vitality, and that a similar power is evolved concurrently with this in the spermatozoon in the contents of the egg, the evolution in both being more or less influenced by temperature, according to the species. These facts are well shown both in the Frog and Toad.

Several pairs of Frogs were collected on the 2nd of March, when the temperature of the season was low, and strong easterly winds prevailed, during which Frogs seldom spawn, except in very sheltered places. As one female had passed some eggs, and believing that the others were equally advanced, I selected a pair, which appeared to be the most mature, for experiment. The fluid from the male was obtained with the greatest ease, in full quantity of a white and somewhat opaque colour. When examined with the microscope it was found to consist in chief part of developmental cells, containing each its motionless immature spermatozoon. Besides these cells it also included many others less far advanced, and in which I was unable to distinguish this body. In addition to these there were some also from which the spermatozoa were in the act of being liberated, besides many very active spermatozoa already set free. These constituted nearly one third of the whole mass. It was evident from these circumstances that the fluid was immature, and not fitted for experiment. Five days afterwards, March 9th, I again examined fluid from this male, which had been kept since the previous examination, in a separate vessel, so that there was no mistake in its identity. The mean temperature of the room during the interval was 42°5 FAHR., and the water in which the Frogs were preserved 40° FAHR., but none of the Frogs had spawned. The fluid was then less dense and of a less white colour, and was composed almost entirely of very active and fully developed spermatozoa, with but very few cells from which these bodies had not

* Article "Semen" in *Cyclopedia of Anatomy and Physiology*, part xxxvi. (January, 1849) vol. iv. p. 504.

been liberated. There were, however, some of which the contents appeared to be perfectly granular, and these I regarded as spermatozoal cells in the earlier stage of development.

The female with which this male had been paired was killed on the 4th, at the time the experiments were to have been commenced. It was then found that the eggs, like the fluid from the male, were immature. The whole of the eggs had escaped from the ovaries into the cavity of the peritoneum among the viscera, and were crowded together in a mass on each side, at the anterior part of the abdomen, and were without any gelatinous covering. A few only had begun to enter the oviduct on the right side of the body, and acquire their envelopes. In most of these eggs the germinal vesicle had already disappeared.

The fluid and eggs of the common Toad (*Bufo vulgaris*) are developed in precisely the same way as in the Frog. On the 30th of March 1851, I examined the bodies of a male and female Toad for the purpose of experiment. The great efferential ducts and vesicles in the male were then entirely empty, no fluid having yet descended into them. But the testes contained a vast quantity of spermatozoal cells, some of which were distended by the spermatozoon, as if in the act of being liberated. But not a single spermatozoon was free, or showed any sign of motion. The whole were still immature, the caudal portion being as yet short and imperfect.

In the female the ovaries were distended with eggs and occupied the whole of the visceral cavity between the intestines. The eggs were of a sooty black colour, with but a slight trace of the grey surface, and appeared to be nearly mature, but had not yet left the ovary. The germinal vesicle of a large size, circular in its outline, and somewhat flattened, still existed in nearly the whole of them.

It is thus evident that the pairing of the sexes, both of Frogs and Toads, takes place before the semen is fully matured, or the eggs have descended into the oviducts, and that there is a near concurrence in the time of maturity in both.

These facts seem to lead to the conclusion, that when, as in some of the fore-mentioned experiments, the male fluid contains a very large proportion of developmental cells, which occasion its white appearance, it is not fully matured for its function; that the first portions of fluid which descend into the efferential vessels are usually immature; and that it is not absolutely necessary that the spermatozoa should be fully formed before the cells escape from the testes. It seems fair then to infer from these facts that the completion of the development of the spermatozoal bodies takes place in the deferential vessels, or in the vesicles, perhaps in both: further, that the quantity of fluid produced, when too great to be contained in the ducts, may become accumulated in the vesicles, to be furnished at the instant it is required; and that there is a near concurrence in the time of maturity in the fluid in the male, and of the eggs in the female, the former being only slightly in advance, in regard to time, of the latter, at the natural period of their encounter; a condition which perhaps may be necessary to the full and healthy fecundation of the whole brood.

3. OF THE OVUM.

The vitality of the eggs of the Frog appears to be of longer duration than that of the fertilizing agent, the spermatozoon. The egg, as formerly shown*, leaves the ovary quickly after, or at the time when its germinal vesicle disappears; but, as experiment has proved, it is not then entirely fitted for its function. It has yet to be enveloped in a thick covering, which it gains in its transit through the oviduct, before it is susceptible of impregnation. Its condition when escaping from the ovary to the oviduct seems to be analogous to that of the spermatozoon when it leaves the testis to enter the efferential duct. Neither the one nor the other has yet attained its full maturity. When the eggs are collected in the dilated or uterine portions of the oviducts they seem to be in a condition parallel to that of the spermatozoa in the lower portion of the efferential ducts and vesicles. When nearly the whole of the eggs are collected in the uterine cavities their maturity is then almost completed; but a period of retention, even in these structures, seems to be necessary to ensure the fertilization of the whole, as some of them,—as I have found by the persistence of the germinal vesicle for a short time, even after the egg has escaped from the ovary into the cavity of the abdomen, as the undeveloped spermatozoal cell passes from the testis into the efferential duct,—are later in their development than others. Further, it may be remembered†, that so far from the contents of the yolk being in a dormant condition at the time of oviposition, as some inquirers have supposed‡, there are changes still going on within it, and which are perceptible to the eye, in the condition of the white surface, for ten or twelve minutes after oviposition; after which they become less and less marked, and soon entirely cease if the egg be not fecundated.

Thus then from a comparison of the state of the fecundatory agent with the body to be fecundated, we might have expected to have found some close coincidence in the retention of their vitality, and this indeed, in a state of nature, seems to be the fact. Spawning seldom or never takes place by the act of the species until each sex has attained its full maturity of function. The eggs are sometimes voluntarily retained by the female Frog, for several hours or days within the uteri, although arrived at maturity, if the proper evolution of spermatozoa in the male be not completed. I have had evidence of this in the fact, that when the union of the sexes has been of long continuance, the female has sometimes passed a very few ova, and then has continued without further oviposition for many hours, sometimes for a day or two, before the mass was expelled, and the function of the sexes consummated. These first extruded eggs have almost always been found to be unimpregnated.

When the eggs have been deposited, the circumstances which affect their fecundation are precisely similar to those which affect the spermatozoa,—the temperature of the surrounding medium, and the length of time during which they remain im-

* Philosophical Transactions, 1851, p. 180. † *Loc. cit.* p. 185. ‡ WAGNER and LEUCKARDT, *loc. cit.*

mersed in water after expulsion from the body. The length of time during which they are susceptible of impregnation has relation to each of these conditions; and these have now acquired additional interest in the establishment of the fact that the spermatozoon penetrates into the envelopes of the eggs, and that, consequently, the rate of expansion of the envelopes has a further direct relation to its function.

The following experiments will illustrate the foregoing observations:—

It may here be remarked, that although, as in my former paper, the date of the experiment is recorded, it is so only for the convenience of reference, and not with any regard to priority of period at which the experiments were made; the details being given only in illustration of the general statements enunciated.

April 2, 1852. Atmosphere 55° FAHR. The first three of the following experiments were made with the eggs and fluid from a pair of Frogs which had been obtained from their natural haunts only three hours before the female was killed, by division of the spinal cord, for the purpose of the experiments.

No. 1. *Eighty-six eggs* passed from the Frog at *one hour and a half* after death, were supplied with fluid from the male with which this female had been paired,—the fluid having been obtained and mixed with water *one hour and a half* before it was employed.

On the seventh day *fifty-two embryos* had been produced from these eggs, and some of them were then escaping from their envelopes.

No. 2. *Thirty-seven eggs*, obtained from the same female, were supplied with a portion of the same fluid as the above, after it had been mixed with water about *two hours and three-quarters*.

Twenty-eight embryos were produced from these eggs at the same time as the above.

No. 3. *Sixty-eight eggs* from the same Frog at *six hours and three-quarters* after death were immersed in the same portion of mixed fluid and water in which the eggs of No. 2 had been fecundated. In this experiment the fluid had been *six hours and three-quarters* mixed with water.

At the end of three days the eggs still had a healthy appearance, but on the seventh day not one had given any evidence of having been impregnated, nor was any embryo ultimately produced. Thus the fluid had lost its fecundatory property, at a temperature of 55° FAHR. between the end of the third and of the seventh hours.

As it was possible that the failure in this experiment was due as much to the eggs as to the fluid employed, it having already been shown that impregnation is sometimes effected with fluid which has been *twenty-four hours* removed from the body,—the following experiments were then made:—

No. 4. *Sixty-nine eggs* obtained from the same female at *one hour and twenty minutes* after death were bathed with fluid procured from another male, and which had been mixed with water *thirty hours* previous. These eggs preserved their regularity of outline for nearly *six hours*, at the end of which time one egg showed traces of

partial impregnation, while the yolks of the others were changing form and becoming irregular. But not a single embryo was produced.

No. 5. *Eighty-one eggs* were obtained from another Frog, a few minutes after it had been killed by division of the spinal cord, and a portion of fluid which had been mixed with water *twenty-four hours and a half*, and kept in a temperature which had sunk from 54° FAHR. to 51° FAHR., but at the time the fluid was employed had again risen to 55° FAHR.—was supplied to them.

This fluid on examination by the microscope was found to contain an abundance of spermatozoa, the whole of which were perfectly motionless, so that I was not able to detect even one which gave any sign of vitality. Yet the eggs in this experiment retained their natural healthy appearance for several hours; but not one was impregnated, nor was even a single embryo produced. On the contrary, after thirteen hours, the yolks of some of them assumed a pyriform shape, while those of others became shrivelled and withered.

No. 6. *Thirty-six eggs* were passed from a Frog, which had been killed about *three-quarters of an hour*, and some fluid which had been obtained and mixed with water *forty-four hours* before was shed over them, and pure water was then added.

The envelopes of these eggs became expanded as under perfectly healthy and favourable conditions; but within two hours of the contact of the eggs with the fluid the yolks of the whole became very irregular, shrivelled, and contracted, and assumed an appearance very similar to that which first results from the application of a strong solution of potass. Not one egg retained its spherical shape, but the whole entirely perished; and it may be needless to add, not an embryo was produced.

These facts then support the statement already made, that only while the spermatozoon continues to give evidence of vitality in its power of motion does it exert any fecundatory influence on the egg. They seem, too, to show that the period during which this vital force is retained by the mature spermatozoon after it is passed from the body of the Frog is perhaps shorter even than what I have stated, and that the limit, in regard to time, depends much on physical influences. In addition to this, they seem to show that after all ocular evidence of power in the spermatozoon is lost, this body is inert, until decomposition has commenced, when its material constituents become injurious and destructive.

The following experiments have more direct reference to the vitality of the egg, and appear to indicate that this is much longer retained than that of the spermatozoon, and is less quickly affected by external causes.

April 2, 1851. Atmosphere 55°.

No. 1. *Thirty-eight eggs* were passed from a Frog killed *twenty-four hours and a half* before, and some fluid which had been obtained and mixed with water *about one hour and a quarter* was then added to them.

Segmentation commenced in some of these eggs in about *four hours and a half*, and on the seventh day *eight embryos* had been produced.

No. 2. *Seventy-three eggs* were taken from a Frog which had been killed about *thirty hours*, and were bathed with fluid which had been *one hour and twenty minutes* mixed with water.

At the end of six hours several of these eggs had become irregular, but one had certainly been fecundated, as one *embryo* afterwards came to maturity.

No. 3. *One hundred and sixty eggs* from the Frog which supplied those employed in the experiment No. 2, were bathed with fluid from another male, immediately after it had been obtained, but not a single egg was impregnated.

No. 4. *Thirty-seven eggs* were passed from a Frog which had been killed *forty-four hours* before, the temperature of the atmosphere during this period having risen from 50° FAHR. to 55° FAHR. These eggs were bathed with fluid which had been obtained about *three quarters of an hour*. At the end of six hours I found, to my surprise, that one egg had become segmented, but that the yolks of the others were flattened, shrivelled, and irregular, as if they had been affected by the solution of potass, or by decomposing seminal fluid. On the seventh day the egg which had been segmented had produced an *embryo*.

From these results, then, it appears that while the spermatozoon at a mean temperature of the atmosphere of 55° FAHR. usually loses its vitality, and is inert in less than four hours, and but rarely is efficient at a longer period after immersion in water, the egg retains its reproductive property for a very much longer time, especially if not removed from the body of the dead animal, only losing it at twenty-four hours after the death of the parent, and occasionally retaining it for forty hours.

The experiment No. 4, in which the latter fact is shown, is interesting, from the circumstance that the same results are produced by the living vibratile spermatozoon on the yolks of the dead eggs, as the majority of these were, as that which is occasioned by the application of dead and decomposing spermatozoa to the envelopes of the living one, as shown in No. 6 of the preceding set; and further, that these results so closely resemble in appearance the first effects produced on the yolks of living eggs by the application of strong solution of potass; thereby showing not only that powerful endosmic action takes place rapidly through the coverings of the egg, but also that the influence of the spermatozoon on the yolk, whatever may be its precise nature, is direct and immediate.

4. ENDOSMOSIS OF THE EGG IN RELATION TO ITS VITALITY.

It has before been shown that the endosmic action of the envelopes of the egg, on immersion in water, is closely connected with the act of fecundation; and we have now further proof of this in the fact that the vitality of the egg may be preserved for many hours if the envelopes be not brought into contact with water; so that the insusceptibility of the egg to become impregnated, after a lengthened period of immersion, is due to a diminution of the expansive property of the tissue of the

envelopes, and the distension of the tissue with fluid, rather than to the loss of vitality in the contents of the yolk.

This view has led me to endeavour to ascertain more precisely than heretofore,—*first*, at what length of time, after immersion in water, the egg usually becomes insusceptible of impregnation; and *next*, to what extent the act of fecundation is accelerated by a given increase of temperature, when the experiments are made with portions of fluid from the same male, and with eggs from the same female, at, as nearly as possible, the same time, all the conditions being similar except in regard to the temperature of the surrounding medium.

With this object, I have recently made two sets of observations consisting each of nine experiments. One set was conducted in a room, the temperature of which was raised artificially to 61° FAHR., and the water in which the eggs were immersed to 60° FAHR.; while the other was carried on in an adjoining room where the temperature was only 55° FAHR., and the water employed 53° FAHR. In order to place the whole of the experiments under as similar conditions as possible, it was necessary that the corresponding experiments in each set should not only be made at about the same time, but should also be supplied with fluid in exactly the same state, and the eggs be immersed in like quantities of water, for the time specified in each experiment, before the addition of the fluid. When the time of immersion had elapsed the water was quickly withdrawn, and the fluid poured over the eggs, and fresh water was immediately supplied to them. As much time was unavoidably occupied in passing the eggs into their respective vessels, and noting the period of immersion, it became necessary to commence the experiments with those which had been longest in water, in order that the fluid supplied to the whole should be as nearly as possible in the same condition, in each experiment, at the moment of its application, and be furnished to the eggs in similar quantities. To ensure this, the fluid was mixed, as soon as it was obtained, with fifteen times its quantity of water, and was employed at about *thirty-five minutes* afterwards. The quantity of this mixed fluid poured over the eggs of each experiment was ten minims by measure, excepting only to the first and second experiments of each set, to which twenty minims were added; but even with this advantage, as was seen, with no more favourable change in the result. After the end of the first hour, the eggs which had remained in the lower temperature were removed to the higher, and the two sets of experiments were then placed side by side to watch the time of commencement of segmentation, as indicative of the more or less accelerated impregnation. The number of embryos subsequently produced showed the ratio in which the eggs had been fecundated after different periods of immersion*. Each set consisted of nine separate experiments, the time of immersion of the eggs being forty-five, thirty-five, twenty-five, fifteen, ten, seven, five, three, and one or two minutes in each.

At the highest temperature, 61° FAHR. atmosphere, and 60° FAHR. water, a tem-

* For the details of the experiments, see MS. paper in the Archives of the Society.

perature which is always most favourable to fecundation, there were no signs that any change had even been commenced in those eggs which had been immersed for a longer period than thirty-five minutes. But it must be borne in mind that the fluid employed had been thirty-five minutes mixed with water, and consequently had begun to lose some of its efficacy. The results of the corresponding experiments at the lower temperature—Atmosphere 55°. Water 53°—were similar, but less marked.

On comparing the two sets of investigations, it was found that no evidence of fecundation occurred in either of them, when the eggs had been immersed during forty-five minutes before the impregnating fluid was supplied to them; and that only the very earliest symptoms of any influence having been communicated to the eggs, after thirty-five minutes' immersion, occurred in those which were submitted to the higher temperature, in the irregular contraction of the yolks; while in each set, both in the higher and lower temperature, some of the eggs, after twenty-five minutes' immersion, showed unequivocal signs of *partial fecundation* in the formation of the *respiratory chamber*, the result of the contraction and depression of the upper portion of the yolk previous to segmentation. But in both sets there were some eggs which, after fifteen minutes' immersion, had become fecundated, and produced embryos. The relative number of embryos produced after immersion for this and shorter spaces of time was certainly greatest in the higher temperature; since, when the total number of eggs employed in the last six experiments in each of the two sets, in which only any embryos were produced, were compared with the number of embryos, it was found that in the higher temperature there were seventy-eight embryos from five hundred and thirty-six eggs; while in the lower temperature, from six hundred and two eggs, there were only seventy-six embryos.

The period at which segmentation occurred in the fecundated eggs of the two sets corresponded also with the above results. In most of the eggs fecundated in the higher temperature segmentation commenced about ten minutes earlier than in those which had been fecundated, and remained during the first hour in the lower temperature.

These results seem to show, that while the eggs are more rapidly affected, they are also more certainly impregnated, and the embryos produced in greater numbers at a higher than at a lower temperature. But it must be borne in mind that the number of eggs fecundated in these experiments can only be regarded as comparative, and as mere approximations to what takes place in a state of nature, the experiments having been made with fluid which had been removed from the body, and considerably diluted with water, more than half an hour before it was employed, and consequently when its efficacy had been considerably diminished.

These facts, then, go to confirm the previous conclusions, that the organic force or vitality of the spermatozoon is of shorter duration than that of the egg; since, if the results of these experiments be compared with those obtained from former ones, it will be seen that the insusceptibility of the egg depends very much more on the endosmic action of its envelopes after immersion in water, than on any loss of vitality:

further, that the non-fecundation of the egg may depend much more on diminished vitality and inefficiency of the spermatozoon, than on the insusceptibility of the egg itself.

5. QUANTITY OF SPERMATOOZOA ESSENTIAL TO FECUNDATION.

On a former occasion* I endeavoured to arrive at some conclusion as to the minimum quantity of spermatozoa necessary to effect impregnation, and to draw some comparison between the results obtained by myself and certain of the more remarkable ones by SPALLANZANI†. It then seemed to me, that although the general results obtained were in accordance with SPALLANZANI's, yet that a larger proportion of fluid, and consequently a greater quantity of spermatozoa, was required than was supposed by him. I have, therefore, made further investigation on this subject, and have sought to obtain some knowledge of the quantity really necessary to fecundate the egg. PREVOST and DUMAS formerly made a similar attempt‡, and M. QUATREFAGES has recently done the same§. The results of the observations by PREVOST and DUMAS, QUATREFAGES, and myself, although differing greatly in detail, coincide with the general results obtained by SPALLANZANI,—that only an exceedingly small quantity of seminal influence, and consequently only a very limited number of vibratile spermatozoa is necessary to effect impregnation. They agree, too, in the fact that the number of eggs impregnated is always much fewer than that of the spermatozoa supplied to them; and, consequently, that there is reason to think that fecundation is not the result of a single, isolated spermatozoon, although it has not been ascertained what is the number actually required. The course I have pursued in putting this curious question to the test, has been different from that of either of the distinguished naturalists mentioned. It has not been that of diluting the impregnating fluid in a very large quantity of water, and then applying a minute drop of the water to the Frog's egg, as done by SPALLANZANI;—nor has it been that of reckoning the number of spermatozoa on a given surface of a micrometer plate, and then immersing the plate in water with a quantity of eggs, and afterwards observing how many of them became fecundated,—the course followed by PREVOST and DUMAS;—nor has it been by estimating the number of spermatozoa in a given bulk of fluid, and then ascertaining the number of eggs fecundated by them, as done by QUATREFAGES with those of the *Hermella* and *Teredo*; but it has been by direct application of the spermatozoa on the point of a pin to the surface of a single egg in a glass cell; and then observing beneath the microscope the number of spermatozoa deposited from the point of the pin, applied in a similar way as to the egg, on a plate of glass. Yet only approximate results can be arrived at by this, or by the other modes of proceeding, although it is satisfactory to find that the general results obtained in this way are in agreement with those obtained by the observers mentioned; and further, that the mode adopted has led to some additional results, which seem to be of value.

* Philosophical Transactions, 1851, p. 206.

† Dissertations, &c., vol. ii. p. 189.

‡ Annales des Sc. Naturelles, tom. ii. 1824.

§ Annales des Sc. Nat. 3^{me} série, tom. xiii. p. 129.

6. TESTS OF FECUNDATION.

I have given some account in my former paper* of a space, or *chamber*, which is developed between the vitellary envelope of the egg and the upper surface of the yelk, within the first hour and a half, subsequent to the encounter of the spermatozoon with the egg; and I have referred to it in the details of these experiments, as occurring in some of the eggs which had been immersed during *twenty-five minutes* before the fecundatory agent was supplied to them, and as marking their having been affected by its influence. The formation of this chamber is one of the earliest and most certain indications that the egg has been more or less impregnated. The earliest symptom of impregnation occurs somewhat sooner than the formation of the chamber, or within the first half-hour, and consists in a marked increase in the degree of undulatory heaving and sinking of portions of the surface of the yelk, motions which take place slightly in the unimpregnated egg, but are increased in the impregnated; although, even in that, they are not sufficiently permanent to be easily recognized as proofs of fecundation. The irregular contraction and shrivelled appearance of the yelks, noticed in the preceding observations, may, perhaps, be due in part to this cause. The formation of the chamber is the result of the contraction and depression of the upper surface of the yelk, occasioned, as there is reason to think, by changes which are going on within the substance of the upper hemisphere of the yelk, around the embryo vesicle, or its progeny, and which changes appear to be due to the operation of the spermatozoon; as I have never observed the chamber in any egg which has not been more or less fecundated. The fact of the existence of a chamber, therefore, may always be regarded as a *proof of fecundation*. It is commenced sooner or later according to the temperature of the surrounding medium, and to the more or less complete fecundation of the egg. It is usually perceptible in about *one hour*, or *one hour and a quarter* after the impregnating fluid has been supplied to the egg, and is distinctly marked in less than one hour and a half, when the eggs are preserved in a temperature of from 50° to 55° FAHR., and its size is increased during the first three hours. It is commenced in a slight depression in the centre of the upper surface of the yelk, at a point which corresponds to the entrance to the central canal, and it takes place much sooner in eggs which have been completely fecundated, even at moderate temperatures, than in others which have been more sparingly affected. I have seen it begun in a fully impregnated egg, in a temperature of 52° FAHR., at *fifty-three minutes* after encounter with very mature spermatozoa. When it has attained its greatest superficial extent it appears like a clear watch-glass cavity, filled with a perfectly transparent, limpid fluid above the dark upper surface of the egg. After the third hour it becomes slightly further enlarged in its axis from above downwards by the formation of a shallow, funnel-shaped depression on the surface of the yelk, the centre of which is the minute orifice seen by PREVOST and DUMAS, and BAER, and this is continuous with the canal above alluded to. I have never found this chamber

* Philosophical Transactions, 1851, p. 187.

absent from any egg which has produced an embryo. Its formation is preliminary to the subsequent cleavage of the yelk. It thus affords, within a very short space of time, a certain *test of fecundation*, and as such is exceedingly valuable in all experiments by artificial impregnation.

7. PARTIAL, OR INCOMPLETE FECUNDATION.

But although the formation of the chamber gives certain proof that the egg has been influenced by the spermatozoon, it is only the actual segmentation of the yelk, carried through the first four stages, which shows that its fecundation has been completed; since the egg may be only partially influenced, and consequently may not produce an embryo, although the respiratory chamber may be formed, and the early stages of segmentation of the yelk be commenced. When this is the case the change seldom proceeds beyond the second or third stage of cleavage, and even then there is usually some irregularity in its occurrence. Sometimes only the chamber itself is developed, and the process then stops; at other times the chamber is completed and a little *white spherical body* makes its appearance, and is but *just perceptible within the entrance to the central canal*; but more frequently a white spherical body, not more than one-fourth the size of the original germinal vesicle, appears on the *surface of the yelk* within the chamber; at other times there are *two of these white bodies*, each of which is scarcely more than one-half the size of the single one, and sometimes the bodies are of a dark or grey colour. In other cases the yelk begins to be divided in the margins of the canal, but does not proceed with its change; or it may go on, as just stated, to the several stages. In other cases the yelk divides very unequally. These instances of *partial impregnation* or *fecundation* may occasionally be produced by application of exceedingly minute quantities of seminal influence, as some of the following experiments will show.

8. DEFICIENCY OF FECUNDATORY INFLUENCE.

(a.) *Pin-point application of the fluid.*—These experiments were commenced in the season of last year (1851), but have been repeated at different periods during the season of the present year.

March 30, 1851. Atmosphere 56° FAHR.—A *single egg* was passed into each of four glass cells, with a small quantity of water, sufficient only to preserve the eggs moist and promote the expansion of their envelopes. Within two minutes afterwards two of the eggs were each touched once with the point of a common-sized pin, which had been so lightly dipped, into a mixture of one part of fecundatory fluid, obtained about one hour and five minutes before, and four parts of water,—as to cover a portion of its surface equal to about one thirtieth of an inch in extent. The two remaining eggs were touched, the first *once* only, and the second *twice*, with the point of a pin slightly curved, and which thus was made to retain a much greater number of spermatozoa in the fluid which adhered to it than the pin with the straight point.

Each cell was then filled with water, so that the egg which it contained was completely immersed. Within the second minute after the eggs had been touched with the loaded pin, there arose from each, at the point of contact, a microscopic bubble of air, the egg which had been twice touched having two bubbles. These were found by subsequent observations to be due to the circumstance of the eggs having been exposed to the air, for a short time before they were touched, and their surface become slightly dried, so that when the fluid was applied to it a small bubble of air became involved and imprisoned in it, before the eggs were immersed in water. This circumstance and its explanation are mentioned simply to show that the occurrence had no relation to the act of fecundation, as at first I had supposed was probable.

I endeavoured to obtain some idea of the number of spermatozoa deposited from the pin point, during its momentary contact with the egg, in these experiments. But the result seemed to give but little hope that either of the experiments would be successful, or that any fecundation could be effected by the application of so minute a quantity of influence, as it was found that the number of spermatozoa deposited on a plate of glass, beneath the microscope, when the pin point was applied to the glass in the same way as to the egg, did not exceed from six to ten or twelve of these bodies. Yet the results of the experiments were watched. Within the first half-hour the eggs were removed from the temperature of 56° FAHR. to one of 65° FAHR. But at the end of ten hours it was found that only in one instance had any fecundation been effected. The grey central spot on the dark surface of the yolk was still present in three of the eggs, and no respiratory chamber had been formed in either of them. But partial fecundation had been effected in the fourth egg, which had been twice touched with the loaded pin point. In this instance the dark surface of the yolk was depressed, and the respiratory chamber had been formed between it and the envelope, and *two spherical opaque white bodies* appeared within it, lying side by side on the middle of the surface of the yolk; but no segmentation of this had taken place, or even had been commenced.

One cause of failure, in these first experiments, appears to have been the diminished efficacy of the fluid, which had been obtained and mixed more than one hour before; as it will presently be seen that even a less proportion of the fluid sometimes is effectual when employed immediately it is procured.

As it was late in the season of last year when these experiments with minimum quantities were commenced, the spawning of the frogs being then nearly at an end, I was fortunate in procuring, on the second of April, a single unspawned pair of frogs from their natural haunts, and thus was enabled to repeat the experiments with the fewest disadvantages. This was done within four hours after the frogs were captured. The fluid in this instance was very mature, was obtained in good abundance, and was mixed with an equal quantity of water, and employed in the experiments a few minutes afterwards.

April 2, 1851. Atmosphere 55°.—*Ten eggs* were passed into a large glass cell, and each egg was touched *once* only with the pin point dipped lightly into the fluid for each egg. This experiment was perfectly successful, as the chamber was formed above the yolk in four or five of these eggs, and at the end of five days three of them had produced embryos. In these instances the pin had been allowed to remain in contact with the egg, and the fluid to drain off it for one or two seconds.

Six eggs were also placed in another cell and each was touched with the loaded pin point when the fluid employed had been obtained about a quarter of an hour. *Two* of these eggs became partially fecundated, as shown in the formation of the respiratory chamber. In each of these the yolk became slightly elongated, a symptom which always precedes the first cleavage; yet they did not undergo further alteration, but with the remaining four were abortive.

Twenty-three eggs were included in a large cell as a further experiment, and each egg was touched once with the pin-point loaded each time, when the fluid employed had been one hour and three quarters mixed with water. *Four* of these also had the chamber formed, and segmentation commenced in them, at the end of four hours and a half; but the majority were only *partially fecundated*, as only one of the twenty-three eggs produced an *embryo*. The fact of this production, however, shows, that an exceedingly small amount of influence, even at a long period after the fecundatory fluid has been removed from the body in which it is generated, is sometimes sufficient to occasion the development of an animated being; and that although a plurality of spermatozoa, supplied to the egg, appears always to be necessary to ensure fecundation, yet that the result seems to depend less upon the absolute given number, or numerical relation of these bodies, to the egg, than on the measure of vitality possessed by those which are brought into actual encounter with it, as a very small number appears to be more efficient when employed immediately after their removal from the body, than a much larger number after a prolonged interval.

(b.) *Pin-head application of fluid.*—The preceding experiments having been made with the smallest quantity of fluid, and consequently with the fewest spermatozoa that could be applied directly to the egg, it was desirable to increase the number of these bodies without applying them in excess, and for this purpose the head of a very small pin, the smallest size used by insect-collectors, was employed, instead of the point of a larger sized pin. An endeavour was also made, as in the previous trials, to ascertain the number of spermatozoa which the head of this sized pin, dipped lightly into fluid, was likely to be the means of conveying to the egg. It was then found that the loaded pin-head deposited on a plate of glass, in the minute quantity of fluid which adhered to it, from at least fifty to one hundred and fifty spermatozoa; a number which was not only fully sufficient to fecundate each egg to which the pin-head was applied, but also other eggs which might happen to come into contact with these in the same cell and water in which the experiment was made.

Six eggs, placed in a single cell, were each touched once only with the pin-head,

which had been dipped into fluid only once for the whole, the time occupied in touching them being about five seconds. Each of these eggs became fertilized, and segmentation commenced in two of them in exactly *five hours*, and in the remaining four in from three to four minutes later. The period at which segmentation commenced having been very carefully watched, and the whole of the eggs having been submitted, during the interval, to the same physical conditions of heat, light, quantity of water and degree of aëration, the result, in regard to the difference of time at which they began to change, seemed to be accounted for only on the assumption that it was due to a greater quantity of influence applied to the first two than to the others; and this opinion has since been more fully borne out by other experiments. The fluid at the time it was employed had been thirty-five minutes mixed with water. On the *fifth* day each of these eggs was producing an *embryo*.

Three eggs were placed in separate cells, and each egg was touched once with the pin-head loaded only once for the three, the fluid employed having been *twenty-three* minutes mixed with water. After *four hours and forty minutes* two of these eggs were undergoing segmentation, and on the fifth day both were producing *embryos*. The third egg was not impregnated.

Twenty-two eggs, in a large cell, were each touched once with the pin-head loaded for each egg when the fluid had been *one hour and forty-seven minutes* mixed with water. Some of the eggs in this experiment were touched on the white surface, others on the dark, and others at the side, or about midway between the white and dark surfaces.

In the same cell with these eggs, but at a little distance from them, were placed eighteen other eggs, which were not touched with the pin-head, but were simply allowed to remain in the same cells with the twenty-two eggs experimented on, and the cell was then filled with pure water. At the end of *four hours and fifty minutes* the chamber had been formed, and segmentation had commenced in each of the twenty-two eggs experimented on. But in addition to these there were also *two* of the eighteen *g* which had not been experimented on undergoing segmentation. These, during the expansion of their envelopes, had come into contact with some of the fecundated eggs, and had also become fecundated by some of the spermatozoa which had been supplied to them. *Five days* afterwards the whole of these *twenty-four eggs* were producing *embryos*! so that not only had the quantity of spermatozoa employed been fully sufficient to fecundate the eggs actually touched, but also others present in the same water with them.

In another experiment, made before the preceding, but with some of the same brood of eggs and same sample of fluid, I placed five eggs in a cell, and touched each once, very lightly, with the pin's head loaded once only for the whole. This was when the fluid had been only *eighteen minutes* mixed with water. The object of this experiment was to learn whether there is any difference in the result when the eggs are touched at different parts of their surface. Accordingly the trial was made by touch-

ing some eggs on the *dark surface*, others on the under or *white surface*, and others at the sides between the two. At the end of *five hours and seven minutes* the chamber had been formed and segmentation was taking place in each egg, and on the *fifth day* each had produced an *embryo*, thus *appearing to show* that it is of little consequence, with reference to fecundation, as to which part of the egg the fecundatory agent is applied to. But this conclusion is subject to certain conditions, as I shall proceed to explain.

9. PORTION OF THE YELK MOST SUSCEPTIBLE OF FECUNDATION.

On attentively examining the preceding experiments, it seemed difficult to understand how it happened that when similar quantities of the same fluid were applied by the pin-head to different eggs of the same brood, at the same time, and the whole of them placed in exactly similar conditions in regard to heat, light and aëration, that some should become fecundated and others not, if, what *appeared* to be shown in the last detailed experiment,—that the egg can be fecundated equally well by the application of the fecundatory agent to any part of its surface, be strictly correct. There at first seemed to be no other way of accounting for the differences in the results, except on the hypothesis, either that it was due to some insusceptibility in the entire egg itself,—which thus presupposed great imperfection in each brood of eggs, a conclusion at variance with observation,—or that it resulted from the mode in which fecundation was attempted.

During the past season (1852) I have put the question, thus raised to a more rigid test, in order to learn whether the egg really is as susceptible of fecundation in one part of its structure as in another.

In order to obtain some decisive results I repeated my experiments on three separate occasions, with eggs and fluid from three pairs of frogs, at nearly similar temperatures of the atmosphere. On the first occasion, April 5th, the temperature was 54° FAHR., on the second, the following day, it was 55° FAHR., and on the last occasion, April 9th, it was 53° FAHR.

In all the preceding experiments the eggs had been allowed to remain in exactly the same position, at the time of applying the fluid, as that in which they had passed from the body of the frog; so that sometimes the dark, and sometimes the white surface was uppermost, and at other times inclined more or less to either side, and occasionally the two were perfectly horizontal. When the fluid was applied by means of the pin's-head to the part desired, some portion of it necessarily flowed over other parts, if the surface which received it happened to be inclined to one side or the other.

But in my more recent investigations care was taken to place every egg in a perfectly vertical position, before attempting to fecundate it, so that in one set of eggs the central part of the dark surface of the yolks was uppermost, and in another the centre of the white surface. These precautions led to very definite results. In the

first experiment, made on the 5th of April, with fluid which had been mixed with water about *one hour and fifty minutes* before, *twelve eggs* contained in a single cell were each twice or thrice freely touched with the loaded pin's-head over the centre of the *white* surface. *Three* of these eggs became *partially* impregnated, as shown in the formation of the chamber in each, and segmentation commenced in one of them, but *no embryo* was produced from either. *Fourteen eggs* in another cell were touched at the same time, and in the same way as the preceding, with the loaded pin's-head, over the centre of the *black* surface of the yolk. *The whole of these eggs produced healthy and well-formed embryos.*

These experiments were repeated on the following day on the eggs of another frog, with fluid which had been obtained only about *twenty minutes* before it was employed. *Fourteen eggs* were each touched *once* only on the centre of the *white* surface, the pin-head being loaded for each egg. But *not one* of these eggs became fecundated, nor was even one of them partially impregnated. In another cell, at the same time with these, *thirteen eggs* were touched on the centre of the *dark* surface, and *four* of them became partially fecundated, but only *two embryos* were produced. The failure in this experiment certainly appeared to be due to the eggs, rather than to the mode of attempting their fecundation.

At the time of making this experiment I placed two eggs, each in a separate cell, with their white surface uppermost, and then filled the cells, the one with nearly *pure* fluid, and the other with equal parts of fluid and water. Each of these eggs became fecundated, and underwent segmentation, although more slowly than usual, but the development of their embryos was not completed, as it became arrested at a particular period, a circumstance which is presently to be explained. Yet these experiments showed that when the egg is completely immersed in fluid its position at the moment has but little reference to the act of fecundation, provided the fecundatory agents are present in good abundance. A further trial was made on the 9th of April, when *six eggs*, placed in one cell, were each *twice* touched on their *white* surface, the pin's-head being loaded each time it was employed. But neither of these eggs produced an *embryo*. *Six eggs*, placed in another cell, were also *twice* touched, like the preceding, on their *dark* surface. The whole of these eggs became fecundated, but only *five embryos* were sufficiently matured as to leave their envelopes. The fluid employed in these two experiments had been mixed with water about *fifty-five minutes* before it was applied to the eggs, and consequently was becoming slightly deteriorated.

The conclusion which seems to be deducible from a comparison of these experiments is,—that when the egg of the frog is completely immersed in water charged with the fecundatory agents, or to which these are supplied quickly after the immersion of the egg, as in the natural fecundation of the species, the actual position of the egg at the moment of its encounter with the spermatozoon, has but little reference to the act of fecundation, every part of its surface having then an equal chance of

becoming affected. In like manner when the fluid is applied directly to any given part of the surface artificially, when the egg is exposed in an empty cell, and the fluid is allowed to gravitate, before the egg is immersed in water, it is of little consequence what part is first touched with it, since it may spread over the surface to the most susceptible portion; there being in reality one part of the egg which is more susceptible than another. The extremes of susceptibility and insusceptibility appear to be, on the one hand the *centre* of the *dark*, or future upper surface of the yelk, and on the other that of the *white* or under surface*.

That these conclusions, deduced from experiment, are correct, seems to be shown in the fact, formerly pointed out†, that the germinal vesicle originally, and its progeny subsequently, at the time of fecundation, occupies the centre, not of the entire egg itself, but of the upper or dark hemisphere of the yelk; and this possibly may be the structure to be fecundated.

These facts deserve consideration in connexion with that of the first changes in the yelk taking place in this hemisphere; first, in the shrinking of the yelk at this part; the formation of the chamber above it, subsequent to fecundation; the *existence of a canal in its centre*, in the margin of which the cleavage of the yelk commences; and, lastly, in this being the portion of the yelk in which the first lineaments of the embryo become apparent.

In connexion with these remarks, it is but just to mention that a former observer, Dr. MARTIN BARRY, has distinctly referred to the changed germinal vesicle as being the structure in which the future embryo, in the Mammalia, originates‡.

10. EXCESS OF FECUNDATORY INFLUENCE.

At the time of commencing the foregoing experiments with small quantities of fluid, I made others of a directly opposite character with an excess of the same. *Four eggs* were placed each in a separate cell, and within one minute afterwards the cells were filled with a mixture composed of equal parts of seminal fluid and water. This was half an hour after the fluid had been obtained. I had expected, from the vast quantity of spermatozoa present in the fluid, that segmentation of the yelk in these eggs would have taken place very quickly; but to my great surprise, although the eggs were preserved in a temperature of 60° FAHR, no segmentation whatever had commenced in them at the expiration of twelve hours! at which time an abundance of spermatozoa adhered to every part of the surface of their envelopes. But no chamber was formed in either of these eggs, as neither of them had been fecundated.

* During the present year I have most fully confirmed these views by numerous experiments, and have proved to my full satisfaction that the *white surface* of the egg is least, and perhaps not at all, susceptible; while of the *dark surface*, that the *centre*, which is occupied by the canal, and its contents, is the fertilizable part.—G. N. April 18, 1853.

† Philosophical Transactions, 1851, p. 176.

‡ Ibid. 1840.

This result appeared to be far more inexplicable than any I had previously noticed. A somewhat similar one had occurred once before, but I had attributed that to some imperfection in the eggs employed, an explanation which did not apply in this case, as eggs from the same female had all been fecundated in other experiments.

Four eggs were then passed from another frog, immediately after death, each into a separate cell, which was filled with a mixture of two parts of water and one of seminal fluid, obtained only a few minutes before it was employed. Two of the eggs were allowed to remain in this mixture in their cells, but the others, after a few minutes' immersion, were well-washed, and supplied with pure water, and the whole were then placed in a temperature of 60° FAHR. At the end of four hours I found the surface of the first two eggs covered with an abundance of spermatozoa; and a chamber had been commenced, and the upper surface of the yolk was depressed, in each, thus showing that some degree of fecundation had been effected, but no segmentation had taken place in either of these eggs at the end of *five hours and a half*, although the temperature of the room had been raised during the interval from 60° FAHR. to 67° FAHR. The sixth hour was almost completed before either of these yolks gave any indication of the probability of this change. Segmentation then commenced, but in a very irregular manner. In one egg the division was not in the centre, but at the side of the yolk, and proceeded no further than to about one-half the extent across the surface. In the other the surface of the yolk within the chamber merely became sulcated twice in the same direction. Neither of these eggs proceeded further with their changes, and of course no embryos were produced. The two remaining eggs did not undergo any change whatever, and consequently had not been impregnated.

Although these failures were still attributed to some immaturity of the eggs, I began to suspect that they might be occasioned by the immersion, or *smothering of the eggs* in fluid of too great a density; yet this suspicion appeared to be so hypothetical as scarcely to merit consideration, when it was remembered that on a former occasion when I had covered eggs with a solution of gum* and afterwards applied the impregnating fluid to them, some of them became fecundated. I was well aware that SPALLANZANI had found in his experiments that the greatest number of frogs' eggs became fecundated when the fluid employed was very much diluted with water†; but I did not then know that M. QUATREFAGES had made an observation‡ which completely coincided with the result obtained by myself. This acute naturalist found that when he employed the pure semen of the sea-worms, the *Hermella* and the *Teredo*, not a single egg was fecundated, and that when he mixed it with only a small quantity of water, so as to obtain "un liquide très opalin," that then only a very small number of eggs showed any traces of fecundation. He ascribed these results simply

* Philosophical Transactions, 1851, p. 236.

† Dissertations, vol. ii. p. 190.

‡ Annales des Sc. Naturelles, 3^{me} Série, tom. xiii. p. 129.

to the circumstance of the fluid being too thick; but as we shall presently find, this, perhaps, may admit of a different interpretation.

My first object having been to learn how small a number of spermatozoa is sufficient to fecundate an egg, it may well be supposed that the facts just mentioned were exceedingly puzzling, and they were rendered still more so by the circumstance that a different result occurred with three other eggs from the same frog, employed at the same time as those in the preceding experiments. Two of these eggs, placed like the others, each in a separate cell, in a mixture of one part of seminal fluid to six parts of water, *two hours and twenty minutes* after the fluid had been obtained, became fecundated. In each of these the chamber was formed above the yelk, segmentation took place, and proceeded regularly, and each egg subsequently produced an embryo. In the third egg the chamber was formed, but the yelk became irregular in outline, and did not undergo segmentation. It had merely three parallel depressions on its upper surface, after which it became strangely altered in form, and was abortive. The cause of this failure seemed to be the same as in the preceding experiments,—an excess of spermatozoa in encounter with the egg, as subsequent experiments tended to show.

Seven eggs were passed into one large cell, and one egg into each of *two* separate cells, and the cells were then filled with a mixture of fluid and water, which was so dense as to appear turbid and opaline. This appearance was in part due to the fluid containing a large proportion of developmental cells. The result in each instance was very marked. The fluid at the time it was employed had been mixed with water about *one hour and five minutes*, and the temperature at the time the experiment was made was 56° FAHR., but the eggs were soon afterwards removed to a temperature of 62° FAHR. At the end of *five hours* the yelks of two of the eggs had become partially segmented, but the changes were continued only in one. The remaining eggs had been more injuriously affected. Their yelks had become *shrivelled and contracted, and resembled withered apples*. In this experiment it was evident that the effect had been occasioned by an excess of impregnating influence, but in what way it was not then easy to understand. The yelks of these eggs looked very like those of eggs which had been in contact with solution of potass, or of those to which stale and decomposing fluid had been supplied.

But it was possible that this change of form might be due to some injury sustained by the eggs in their removal from the body of the frog, or to some accidental cause in their removal to the cells, before fecundation. In order, therefore, to remove all doubt in this respect, another trial was made with two other eggs, which were so carefully passed into cells as to avoid all suspicion of injury to them, and the cells were then filled with mixed fluid like the preceding. The yelks of each of these eggs also changed their form, and one of them became irregularly contracted and pear-shaped. Of the whole of the eggs employed in these experiments only *one* underwent segmentation, and produced an *embryo*. The others were not fecundated. It was

thus evident that an excess of fecundatory fluid is as unfavourable to the production of the embryo as a deficiency.

11. DEFICIENCY AND EXCESS OF FECUNDATORY INFLUENCE COMPARED.

The results of these experiments with the fecundatory agent in excess require to be further explained, and also to be compared with those obtained with deficiency of the agent, in order that the nature of both may be properly understood. If this be done it will be seen that fecundation of the frog's egg depends very much on the *physical influence of temperature*; and that this has a close relation with the intensity of the force, or the degree of vitality evolved in the spermatozoon, at the time of its application to the egg; and probably also with the influence which the spermatozoon supplies through a greater or less amount of material substance to the body to be fecundated, at the time of its encounter, as in the following experiments made in March 1852.

The experiments by pin-point application of fluid show that only a very small amount of influence is actually required to set up those changes in the yolk which result in the formation of the embryo; and other observations have convinced me that the result is the more certain at a slight increase than at ever so slight a diminution of temperature. The effect of the application of minimum quantities is most certain when the influence has only very recently been obtained, and consequently while still endowed with its greatest degree of vitality. But even at that time a given amount is required, and if such be not supplied the result is incomplete, and partial fecundation only is effected. First then in regard to minimum quantities.

Six eggs were placed each in a separate cell, and three of these were touched *once* only with the pin-head loaded with fluid obtained about twenty-five minutes before, while the other three were each touched once with the fine pin-point loaded in like manner. Yet although there was only an interval of one minute between the application of fluid to the two sets of eggs, two of the former by pin-head application had the chamber developed, and segmentation commenced at the end of *three hours and forty minutes*; while one egg only of the three supplied by pin-point application began to be segmented in *three hours and forty-eight minutes*, although the whole were placed under precisely similar conditions in regard to light, heat, quantity of water and degree of aëration. The third egg of the first set was not impregnated, neither were the two remaining ones of the second set. Thus there was a difference between the two sets of eggs, not only in the extent to which they were fecundated, but also in the *rate of the accomplishment of fecundation* to the extent of *eight minutes*, in about three hours and three-quarters. There was also a perceptible difference of time in the formation of the respiratory chamber of the two, after the first hour, as was remarked by a distinguished physiologist who was present with me when these comparative trials were made. The two eggs of the first set, and the single egg of the second, which underwent segmentation, afterwards produced embryos; the remaining eggs were abortive.

A second comparative trial was made on the 15th of March. *Three eggs* in separate cells were each touched once with the fine pin-point loaded with fluid which had been obtained only twenty minutes before. The temperature at the time of the experiment was 56° FAHR., but the eggs were removed within a few minutes afterwards to a higher temperature, which was gradually increased to $64^{\circ}.5$ FAHR. Segmentation took place in one of these eggs in *three hours and thirty-nine minutes*, and this egg afterwards produced an *embryo*. But the remaining two were not impregnated. Three other eggs in separate cells, were, at the same time as the preceding, each once touched with the loaded pin-head, and two out of the three underwent segmentation at the end of *three hours and thirty-three minutes*. In a further experiment, made at the same time, *sixteen eggs* included in one cell were touched with the loaded pin's-head when the fluid had been obtained about half an hour before. *Four* of these eggs underwent segmentation in *three hours and fifty-three minutes*, and afterwards produced *embryos*, the remaining eggs being unproductive. Another experiment was made with *nineteen eggs* in a single cell, by pin-head application of the fluid, and eleven of these were fecundated and began to be segmented in *three hours and forty-two minutes*, and ultimately produced embryos, the remaining eight being sterile. Some of these failures were probably due to the circumstance that the fecundatory fluid was not applied to the most susceptible part of the egg, yet the general results sufficiently show not only that fecundation is more surely and quickly effected at a moderately high than at a low temperature, but also that it is so, more or less certainly and quickly, in proportion to the amount of influence supplied.

These facts lead us to some further consideration of the results of the application of definite quantities of the fecundatory agent to the egg, as affecting the development of the embryo, and *possibly also as influencing both the evolution of its physical structure and psychical condition*. I have little hesitation in believing that whatever be the precise nature of the influence communicated by the fecundatory agent to the egg, that it is only after full and complete impregnation that perfectly normal and healthy embryos are formed, and ultimately attain to the maturity of the species. And yet we have already seen that when there is great excess of the agent, either that no embryo is produced, or that its development is not completed.

A few days after making the last-mentioned experiments I repeated them with another object in view, and then obtained a result which had occurred on previous occasions, but being less distinctly marked had been almost overlooked.

About *twenty eggs* had been placed in separate cells, and fecundation attempted by pin-head application of fluid which had been obtained from a partially exhausted and debilitated male, at the end of the season, and which had been employed from want of a more healthy individual. The fluid, when examined by the microscope, was found to contain very many perfectly motionless spermatozoa, besides a large quantity of cells. The inefficiency of this male was inferred from the circumstance that the

female was prepared to spawn, as was proved by her having passed a few eggs; but these remained unimpregnated, and she still retained the remainder within her for nearly a day, at which time the male was removed for the experiment. The employment of this male was thus accidental, it being the only one I then possessed.

The fluid was applied to the eggs within a quarter of an hour of its being obtained, and the eggs were at once placed in a temperature of 61° FAHR. I remarked that development appeared to go on more slowly than usual, although most of the eggs underwent segmentation, and *fifteen* of them produced embryos, which were placed together in one vessel. A few days afterwards I was surprised to find that two of these embryos were greatly malformed. One had a short falcated tail narrowed at its base, where it was not broader than at its extremity, and the tail of the other was scarcely more than a short narrow stump, so that the embryo had much difficulty of locomotion. A few days later three other of the embryos became distorted, and one of them died much altered in form at the period of absorption of the external branchiae.

These circumstances are recorded merely as simple occurrences which point to the necessity for further experiment on this curious and important subject, and not as results from which any very positive conclusions can be deduced. Nevertheless, I may add, that I think I have before observed that embryos which have been the result of fecundation with very small quantities of fluid, are usually smaller than others which are produced from full and natural impregnation.

The effect produced by immersion of the egg in pure fluid is as curious as the results above mentioned obtained from minimum quantities, and at first it is less easily to be understood. I can find no more expressive term by which to indicate it than that of *smothering*, and this, perhaps, may be found to be a more literally correct expression of the fact than at first is apparent. It has already been shown that free aëration is most essential to the development of the embryo, and I believe it is equally so, in a minor degree, to fecundation. Thus while the egg may be fecundated in a dense fluid, composed of equal parts of seminal fluid and water, its changes usually take place much more slowly than when the proportion of water is considerably increased; while if it be immersed in pure fluid, and be allowed to remain in that for some hours, it may still be fecundated, and its changes be commenced, but these will not proceed to the full development of the embryo. If the egg when immersed in pure fluid be retained in a moderately low temperature, then the development of the embryo may go on slowly for a time, but with less rapidity than when the egg is freely aërated in pure water. But if, on the other hand, it be retained in seminal fluid, at a high temperature, then it usually happens that although the respiratory chamber be formed, as the result of fecundation, segmentation will proceed slowly, or irregularly, and the embryo, which is begun to be formed, perish. That want of proper aëration is in part the cause of this infertility seems to be shown in the following experiments.

April 5, 1852. Atmosphere 54°.—Two eggs were placed in separate cells, which were immediately filled with nearly pure fluid. In about one hour afterwards the respiratory chamber was beginning to be formed in both these eggs, but its progress was slow. One egg was then washed as completely as possible with a powerful jet of water, thrown upon it by a syringe, and the cell was then filled with pure water, while the cell with the other egg was replenished with more of the fecundatory mixture. After the addition of fresh water to the first egg its envelopes began to expand, and it was then seen that the substance of these coverings was *filled with spermatozoa, lying in every direction and so crowded around the envelope which immediately invests the yolk, as to render the whole semi-opake*. The envelopes of the second egg were much more opake than those of the first. Yet the respiratory chamber was developed, after a lengthened interval, in both, but earlier in the egg which had been washed and placed in water than in that which remained in the fluid. At the expiration of twelve hours the second egg was washed and placed in pure water like the first. Both eggs were preserved, from within an hour or two after their first encounter with seminal fluid, for four days in a temperature which ranged from 59° FAHR. to 64° FAHR. At the end of three days and a quarter, or about seventy-eight hours, in this temperature the development of the embryo had advanced so far in the first as to the union of the laminae dorsales, and the narrowing and elongation of the body, a stage of development at which the function of aëration is becoming more energetic in the evolution of ciliary action. At this stage of formation the development of the embryo became arrested, although the egg was properly supplied with water, and from this period it decayed. The second egg, which had remained twelve hours in the mixed fluid before it was supplied with fresh water, perished much earlier, and before there were any distinct evidences of the formation of an embryo.

On other occasions I have found that the egg does not become fertilized at all, but the yolk contracts irregularly, and resembles the yolk of eggs affected by solution of potass.

These are the usual results of immersion in undiluted fluid, which, contrary to what occurs with minimum quantities, appears to be most prejudicial to the egg, in its pure state, *when the temperature of the surrounding medium is much increased*; so that the cause of failure in these cases of *excess* may reasonably be attributed, in great part, to a smothering of the egg, through impeded aëration; accelerated, perhaps, by some chemical change, through increased temperature, in the remains of the spermatozoa, with which the envelopes of the egg are crowded in their interior.

12. THE MOTION OF THE SPERMATOZOON IN RELATION TO ITS FUNCTION.

The possibility of the fecundatory function being in some way connected with the motion exhibited by the contents of the pollen in plants, and with that of the spermatozoa in animals, has not escaped the consideration of the best observers. Mr. Brown, the most distinguished of botanists, many years ago, made this the

subject of his particular investigation*; and M. BRONGNIART†, SCHLEIDEN‡, NÄGELI§, GRIFFITH||, and more recently SUMINSKI¶ and HENFREY** have done the same. But although neither of these able inquirers succeeded by direct experiment in proving that the motion of the particles of plants is essential to the act of impregnation, M. HOFMEISTER††, and very recently also Mr. HENFREY‡‡, have noticed facts in regard to that of the spermatozoid filaments discovered by NÄGELI and SUMINSKI, in the Cryptogamia, which seem to show that, in plants, it is of great importance to the function of these bodies. The motion of the spermatozoon in animals has equally attracted the attention of zoologists. PREVOST and DUMAS§§, as already stated, SIRBOLD, MÜLLER, WAGNER, KÖLLIKER, BISCHOFF, QUATREFAGES, and especially WAGNER and LEUCKARDT, have studied it attentively; but so intricate is the inquiry concerning its nature and import, that the last two authors dismiss the consideration of the question without arriving at any conclusion, and state that they do not venture to decide|||. Heretofore I regarded impregnation as being commenced by transmission from the spermatozoon on the surface of the egg, to the contents in the interior, of some influence characterized by motion. But I have regarded this motion as being only the visible indication of a *peculiar force*, or *form of vitality*, in the impregnating agent, the spermatozoon, by which it is destined to arrive at, and is to expend on the object to be fecundated, and the effect of which is to *strengthen*, to *augment*, and possibly also to *modify* the nature of the formative changes, which are going on in the yet unimpregnated egg, *per se*; but which will subside, and soon entirely cease, if not reinforced through the agency of the spermatozoon. Nevertheless, I have not been prepared to assent to the view that simple contact of the spermatozoon, *even with the vitelline membrane*, is sufficient to complete the changes which result in the formation of the embryo (see p. 233). The powerful endosmic action of the envelopes of the ovum, at the time of oviposition, is opposed to this conclusion; since, if simple contact

* A brief account of microscopical observations, made in the months of June, July and August, 1827, on the particles contained in the pollen of plants, and on the general existence of active molecules in organic and inorganic bodies, by ROBERT BROWN, F.R.S., etc., 8vo. July, 1828; also additional remarks on active molecules. (*Id.*) July, 1829.

† Recherches sur la génération et le développement de l'embryon dans les Végétaux Phanérogames (Notes). Annales des Sciences Nat. tom. ix. 1828.

‡ Grundzüge der wissenschaftliche Botanik.

§ SCHLEIDEN und NÄGELI's Zeitschr. für Wiss.-Botanik; Heft i. 168; Zurich, 1844.

|| Transactions of the Linnean Society of London, vol. xx.

¶ Zur Entwicklungsgeschichte der Farrnkräuter, 4to. Berlin, 1848.

** On the development of the ovule in *Orchis morio*, Transactions of the Linnean Society of London, vol. xxi., and Proceedings, vol. ii. p. 27, April 8, 1849.

†† Untersuchung des Vorganges bei der Befruchtung der Enothereen, Botanische Zeitung, v. 785, 1847.

‡‡ Annals and Magazine of Natural History, vol. ix. June, 1852; Transactions of Linnean Society, vol. xxi. Part II.; also Proceedings, June 17, 1852, vol. ii.

§§ Annales des Sciences Naturelles, tom. ii. 1824.

||| Article "Semen," Cyclopædia of Anatomy and Physiology, vol. iv. par. xxxiv. p. 508.

or encounter of the spermatozoon with the ovum were sufficient, it might be expected, as has been well remarked*, that the influence of the spermatozoon of any animal of the same class would be competent to effect the impregnation of any species.

Yet all the phenomena connected with the origin and death of the spermatozoon seem to be in accordance with the view, that its *motion* is essential to its function. Whatever be the relation of this motion to its peculiar faculty, it is evident that motion is intimately associated with, and dependent on, its material composition, and structural development. In the Frog, the spermatozoa are usually completed very early in the season, when the animals begin to emerge from their hybernacula, in the beginning or middle of February; but, as we have seen, they only begin to pass into the efferential ducts from the testicle, at the time when the pairing of the sexes is commenced; from which time, to that of spawning, a period of from ten days to a fortnight or three weeks, according to the temperature of the season, they are more fully matured, and acquire greater vibratory power, and are collected in the vesiculae seminales for expulsion at the instant after oviposition. In the Toad the spermatozoa are developed at a later period of the season, but at a relatively corresponding period in the life of the animal. The male Toad, like that of the Frog, usually emerges from its hiding-place a few days, or a week or two earlier than the female. I have taken the males in the middle and at the end of March, at which time I have not been able to detect any seminal fluid in their vesiculae seminales; yet they are then exceedingly salacious and disposed to pair, and I have sometimes found the contents of the reproductive organs in a similar state, even after the sexes have been for two or three days in union. The testicles, nevertheless, are then filled with an abundance of spermatozoal cells in the course of development, and also with a great quantity of spermatozoa, each still included in its vesicle of development; but as yet immature, *motionless*, and with only a very short caudal extension from its thick cylindrical body. Besides these there are usually a few spermatozoa, more matured than the rest, which exhibit movements while still retained within their cells, which are enlarged, and from which they are soon to be liberated.

Thus motive power in the spermatozoon is coincident with the completion of its structure and composition, and as such, may fairly be regarded as *essential to its function*. In the Frog the motion of the spermatozoon is most intense and persistent at the full period of connubiality. On the other hand, I have constantly noticed in all my experiments on artificial impregnation, that where impregnation has not been effected, all the conditions being favourable to it, or when I have found by trial that the male fluid *has ceased to be efficient*, that then nearly the whole, or perhaps all of the spermatozoa, have been perfectly motionless, and apparently dead. This is also the condition in which I have usually found the few spermatozoa which are retained in the reproductive organs a week or two after pairing, when the male Frog and Toad may be regarded as in the state of aged individuals, the season of reproduction

* WAGNER and LUKARDT, *loc. cit.* p. 508.

having passed, and the function being fulfilled. Thus too, I have noticed that, when from accident, but more especially when from reduction of the temperature in the surrounding medium, the season of spawning has been greatly retarded, the impregnating power of the male is much diminished, and perhaps is almost exhausted, through constant shedding of the spermatid fluid, which, I have found, often takes place when the oviposition of the female is delayed, and the individuals are disturbed or interfered with. The female is then forsaken by her partner, and when this occurs it rarely happens that the connubial intercourse of these two individuals is recommenced. When this separation has taken place, there is usually but a small quantity of fluid remaining in the male organs, and even in that, the number of spermatozoa is considerably diminished, and *their power of motion is exceedingly feeble*; while the quantity of molecules and cells is increased. When several days, or a week or two have elapsed, there are not only fewer spermatozoa, but those which remain are much more feeble in action. This is exactly what occurs also in the Toad. On the sixth of June I found that the testes and efferential ducts in a male Toad, which had been kept from pairing during the whole season, were still filled with spermatozoa, together with a very small quantity of liquor seminis with active molecules moving in it; but that, though the spermatozoa were in full abundance, nearly the whole of them were entirely motionless, while the motions of the few which still gave evidences of vitality, were exceedingly feeble, whether the spermatozoa were examined simply in the fluid portion of the semen, or whether they were mixed with water, in which, as is well known, the motions are always at first greatly increased.

A similar reduction in the number of the spermatozoa and diminution of their motive power, appears to exist in animals which have become exhausted through long confinement or want of food; at least, if we may so judge, from a few observations on the Tritons. A *Triton palustris*, which had been captured on the seventh of May, and accidentally confined without food till the sixth of June, was examined immediately after death. The efferential ducts were well-filled with spermatozoa contained in a distinctly perceptible quantity of liquor seminis. When the spermatozoa were examined, without the addition of water, their motions were regular, but apparently very much slower than usual, being uniform and undulating, without that peculiar rapid ciliary action of the spirally twisted tail, which is so constantly referred to as characteristic of the spermatozoa of the Tritons and of some other Amphibia. When water was added, the motions were immediately accelerated, and the tail, which before was merely flexed, and almost longitudinally extended from the body, became folded and entwined around it, and its rapid ciliary movements were commenced. But these gradually subsided within a very few minutes. In another specimen, which had been captured at the same time as the preceding, and confined under similar circumstances, but which, at the time of examination, had been already dead for more than twenty-four hours, scarcely any spermatozoa remained in the testes, or in the efferential ducts. There was a great quantity of cells, with granular nuclei, in

the body of each testis, but scarcely a single spermatozoon. The few spermatozoa which remained were most of them dead, and decomposition appeared to have commenced in them, and there was a large proportion of active molecules. Amongst these were one or two spermatozoa still in action, and the molecules were accumulated around them by the attraction of the current in the fluid, induced by the ciliary action of the tail, and were propelled onwards, frequently in a spiral direction, from the base of the tail to the anterior extremity of the body; thus showing that the motions around the spermatozoon are due to the action of the filamentous tail, flexed upon, and twisted spirally round the body; in accordance with the observation of WAGNER and LEUCKARDT.

But it has been remarked, in opposition to the view, that power of motion in the spermatozoon is essential to its function, that motion is not detected, or but very faintly, in many instances in which the spermatozoa are united into simple and uniform cords, as they often are in the deferential vessels of insects; and that then only a slight waving, or trembling of the mass, the consequence of hygroscopic conditions induced by the fluid around, is, under such circumstances, observed. An aggregation of the spermatozoa into cords takes place in the Tritons as well as in insects. When the semen is expelled by the Triton at the time of conjugation, it is composed almost entirely of white cord-like, flocculent masses of spermatozoa, which, at that moment, exhibit only the faint undulatory motion noticed in them while still within the deferential vessels. Yet, immediately after these masses are ejected from the vessels, and are in contact with water, at the moment of being received into the cloaca of the female, as I shall hereafter have occasion to show,—the motions of the spermatozoa are not only greatly increased, but that peculiar vibratory ciliary action of the tail, which, while the spermatozoa are still within the seminal ducts is very indistinct, and perhaps can scarcely be said to occur—is immediately set up, and the motion of the body of the spermatozoon is changed from the undulatory action before observed, to the jerking or watch-spring movement pointed out by observers, and thought to be the motion peculiar to the spermatozoon of this class of animals.

Thus then, the fact of only a slight wavy motion of the spermatozoon being perceived in some cases, ceases to be of importance, as an argument against the view that motion is closely connected with the function, since it is evident that the true spermatic force is not set up until the spermatozoa have become more or less separated from each other, through their residence in some fluid vehicle. The movements of the spermatozoon in insects are very similar to those of the spermatozoon of the Triton, and the circumstances which affect both are in full accordance with the fact of the existence of, and the necessity for, a distinct liquor seminis, as part of the normal composition of the seminal fluid, in the Mammalia; while this portion of the fluid is not needed, and consequently is almost entirely absent in the Amphibia, Fishes and most Invertebrata. A comparison of the facts in the two divisions of animals seems to point to the true nature and use of the liquor seminis, as being that

of a mere vehicle for the ready transmission of the spermatozoon to the ova, and as allowing greater freedom of motion to these bodies.

It is true, as WAGNER and LÜCKARDT have stated, that but little motion can be observed in the spermatozoa of Insects when they are united into cords; yet this seems to be due rather to their union into these masses, and to have reference to their mode of ejection from the body of the male, as in the Triton, and is known to take place during their collection in the deferential vessels, than to any real absence of a power of motion in them. This is not their condition when brought into contact with the egg at the time of impregnation. When the fluid of the male insect, during pairing, is passed into the spermatheca of the female, where it may be destined to remain for an indefinite time, the spermatozoa compose the chief portion of it, as in the Tritons; but, during their residence in the spermatheca, the spermatozoa are mixed with a fluid, which is supplied by a gland attached to that organ, and which becomes to them a vehicle like the liquor seminis, and allows of their separation and independent motion, and thus appears to answer the purpose of the true liquor seminis in the Mammalia. At the period when the ova are descending from the ovaries, the movements of the spermatozoa become more distinct; and when these bodies are brought into contact with the egg, as it passes the outlet of the spermatheca, they become more isolated, and their movements more intense, as I have seen in the case of the Orthoptera. From these circumstances I am led to believe that some degree of motion will ultimately be observed to mark the perfect condition of the impregnating agent in all animals. The facts already mentioned coincide with this view, and point to the probability that the degree of impregnating force in each individual may perhaps be indicated relatively by the degree or intensity of motion in the spermatozoon, and the duration of this force by the length of time which the spermatozoon continues in motion.

The established fact, that a difference in the structural conformation of a body is the invariable result of a difference in the relations, proportions, and composition of its material constituents, has always afforded reason for presuming that some material influence may be transferred from the substance of the spermatozoon to the contents of the ovum, at the time of impregnation. The function of impregnation appears to be one of definite relations and proportions. Thus we have seen, when only a few spermatozoa were applied to the ovum on the point of a pin, that full impregnation was but rarely effected. In most instances of such limited application, the yolk underwent only partial segmentation, and its changes were then gradually arrested, and no embryo was produced. If, however, the pin point which had been charged with spermatozoa, instead of being applied to the ovum for an instant only, was allowed to remain in contact with the egg for a second or two, and thus by capillary attraction became drained of the spermatozoa which adhered to it, and, as a consequence, thus made to deposit a greater number of these bodies, which were not afterwards attempted to be removed or destroyed, then impregnation was sometimes

completed, and an embryo became developed. But if, instead of the spermatozoa being thus supplied in a minimum quantity from the *point* of a pin, they were supplied in greater quantity from the *head*, then impregnation was almost always effected; and this result was rendered the more certain by allowing the head to be drained of the adhering bodies for a moment or two, as from the point, by which a much larger quantity of the impregnating agents was deposited. In these instances it was rare that impregnation was not effected. These results appear to show, that, whatever may be the precise quantity necessary to effect healthful impregnation, it has some definite relation to the effect to be produced in the contents of the ovum—that a definite quantity of spermatozoa, or spermatic influence is required to fecundate, and that the perfection of fecundation has relation to the degree of impregnating influence. They seem to show, also, that fecundation is not the simple result of the penetration into the egg of a *single isolated spermatozoon*, but probably of some definite number of these bodies, or of a definite amount of influence supplied through their encounter. We are thus led to perceive that the same law of relation between cause and effect,—between a definite amount of influence expended, and definite results,—which has long constituted the basis of our knowledge respecting chemical affinities, and which is now being demonstrated, as that also of the other forces of inorganic nature, may equally pervade and control these material combinations among the organic affinities. Further, the observations, now mentioned, seem to put to rest the last remaining question respecting the independent animality of the spermatic bodies, and to show that these, like the cilia, are mere elementary parts of the adult male organization,—as many physiologists believe*,—as the contents of the ova are of that of the female.

All my experiments on the egg of the Amphibia serve to show, that even with a definite quantity or number of spermatozoa, no impregnation is effected, if, before the spermatozoa are brought into contact with the egg, they have all ceased to exhibit that motion which constitutes their marked characteristic. Thus then, it seems that, independent of the important question as to whether these bodies yield any material substance for combination with the constituents of the egg, through the usual chemical affinities of matter,—the quantity of influence to be supplied to produce the healthful result is definite, and that, whatever be its nature or essence, it is always characterized by a definite degree of *motion* in the spermatozoon. Further, all the observations I have made on the spermatozoa tend to show that their motion is rendered more vivid and intense by an increase of heat, but that, in proportion to such increase, it is so much the sooner exhausted; as, on the other hand, it has long been known that it is diminished by reduction of temperature, but increased in duration. Many of the observations lead to the view, that the power, in a given quantity or number of the spermatozoa, to effect impregnation, is more in proportion to the amount, *quantity, or intensity of the motion* exhibited by these bodies than to their actual

* KÖLLIKER, SIEBOLD, MÜLLER, WAGNER, &c.

numbers. Thus the spermatozoa, like the ova, are more efficient at a given temperature, within the first two or three minutes after they are passed from the body, than at a later period. At that time there is not only a greater number of spermatozoa in a given quantity of fluid in a state of activity; but the whole of them exhibit a greater intensity of motion than after they have been for some time mixed with water. In the Toad, which, in this country, as already stated, pairs at a few weeks later in the season than the Frog, and which seems not only to require a higher temperature of the surrounding medium, but also a relatively higher temperature for the development of the ova, the spermatozoa are most active, and most fitted to impregnate at the instant of expulsion; as is shown in the mode of intercourse of the sexes in that animal, and in the fact that, if the fecundatory fluid be not immediately applied to the strings of ova as these are passed, the unsprinkled ova are infertile. The oviposition of the Toad is an exceedingly slow process, and usually lasts, as SPALLANZANI observed, from ten to fifteen hours: in one instance I found it continued during seventeen hours, but it was completed in others in eight or ten. The act of impregnation, therefore, is necessarily also prolonged. But the length of time during which the spermatozoa continue efficient to impregnate after removal from the body, is shorter than in the Frog. SPALLANZANI found that, at a temperature of 81° FAHR., it does not exceed fifteen minutes. I also have noticed that the motions of the spermatozoa cease much earlier than in the spermatozoa of the Frog.

Thus the conclusions to be drawn from the facts of natural impregnation in the Toad, fully agree with those deduced from artificial impregnation in the Frog, and seem to establish the view, that while an increase of temperature is required for the fulfilment of the reproductive function in that animal, and to maintain the efficiency of the spermatozoa, the fecundatory force of the agent is of shorter duration, and corresponds to its more early cessation of motion.

13. PENETRATION BY THE SPERMATOZOON IN EFFECTING FECUNDATION.

The penetration of the spermatozoon into the substance of the egg, or even into its envelopes, has been so much disputed, and so repeatedly denied, that it is only on what may perhaps be regarded as indisputable evidence, that any one ought to re-assert it. Up to a very recent period I could not, myself, admit it as probable; because, throughout my investigations, I had not been able to detect any appearance in the fecundated egg or its envelopes, either of the Frog or Newt, which rendered it likely that the spermatozoon penetrates into or through them. Yet the fact of penetration was stated by the older naturalists, not so much, perhaps, from any decided proof of its occurrence, as from a confidence that it was only in this way that the function of the spermatozoon could then be understood. LEEUWENHOEK, and those of his day, not only believed that the spermatozoon penetrates bodily into the substance of the egg, but also that it becomes the future embryo. In later times, M. PREVOST having, with M. DUMAS, seen the spermatozoon within the gelatinous envelope of the

egg of the Frog, as they show, conceived that this body becomes,—not the embryo itself, but the foundation of the nervous system of the embryo. Since then, Dr. MARTIN BARRY announced to the Royal Society that he had *seen* spermatozoa within the substance of the egg of the Rabbit; through the envelopes of which, he has stated, that there is a natural perforation or cleft at the time of fecundation*; a view which has met with much opposition, as no physiologist has hitherto (1852) verified his observation with regard to the existence of a perforation or cleft in the egg-envelopes; or has announced that he has *seen* the spermatozoon *within* the ovum of that animal. Indeed so general has been the opinion that there is no penetration by the spermatozoon, even into the envelopes of the egg, notwithstanding WAGNER's† announcement that he had seen spermatozoa within the envelopes in the eggs of Fishes, that one of the most recent investigators, M. QUATREFAGES, in alluding to MM. PREVOST and DUMAS' views, observes, that "it is useless to allude to the question of penetration by the spermatozoon, as he believes that the only living author of the theory has himself renounced it‡." But very recently it has been announced, in a paper on the *Ascaris Mystax*, by Dr. NELSON, communicated to the Royal Society§, that the spermatozoon in *that* animal *does penetrate* into the substance of the yelk; and a somewhat similar account of that of the Earth-worm was formerly given by Dr. ARTHUR FARRE||. Yet, with all due respect for the observations of these able investigators, I must still have hesitated to admit the fact of any penetration, even into the envelopes of the egg, had I not more recently been convinced of this fact by direct observation, in correction of my former opinions, in so far as relates to the penetration of the spermatozoon into the envelopes of the eggs of the Frog, and its arrival at the vitelline membrane. To them, then, be all honour on this subject, while I subjoin my testimony to a fact which I had heretofore failed to observe.

It was during the month of March last (1852), while making experiments on the Frog's egg by artificial impregnation, before some scientific friends, that certain appearances were noticed beneath the microscope within the expanded jelly of the egg, which led to a suspicion of the probability of penetration by the spermatozoon. Heretofore I had employed, in my observations, either a deep glass cell, in which the egg was completely immersed in water, or the common object-glass of the microscope. In the experiments now referred to, a shallow glass cell was employed, which was capable of containing only a single egg. This, while it admitted light freely around the egg, was too shallow for its complete immersion, and the egg was therefore covered by a drop of water, into which the object-glass of the microscope was passed,

* Philosophical Transactions, Part II. 1840, p. 533, Plate XXII. figs. 164, 165 and 167. Ibid. Part I. 1843, p. 33.

† Elements of Physiology (English Edit. by Dr. WILLIS), note, p. 74, 1841.

‡ Annales des Sciences Nat. 3^{me} Série, tom. xiii. 1850.

§ Proceedings, June 19, 1851, vol. vi. p. 86. Philosophical Transactions, 1852.

|| See Dr. CARPENTER'S Principles of Human Physiology, 1st edit. 1842, p. 617.

while viewing the egg, instead of the observation being made through the double medium of air and water. This was the method of examination in all the subsequent experiments.

The appearance which first led me to suspect that spermatozoa do penetrate into the envelope, was produced by an acicular body, which seemed to have its narrowed extremity in near proximity to the vitelline membrane. The direction of the longitudinal axis of this body was in a line with the centre of the yolk. But what appeared to be its larger end was farthest removed from the yolk; and this circumstance seemed to show that the appearance could hardly be due to the presence of a spermatozoon, which, if the observation were correct, must have penetrated in a direction the reverse of that of its usual motion.

It was necessary, therefore, that the suspicion raised by this observation should be settled. I had again and again found, as before shown, that the egg of the frog may be impregnated, under certain conditions, by the direct application of spermatozoa to almost any part of its surface, and this enabled me to put the question of penetration to the test. In the first trial, an egg was placed in a single cell, and immersed in water for *one minute*—the water was then removed, and the egg touched on one point only of its surface with the head of a pin, loaded with the fecundating fluid, which had been obtained and mixed with water about two hours before. I had expected that on watching the egg beneath the microscope, from the instant of contact with the pin's-head and the re-filling of the cell with water, to have been able to detect the spermatozoon during its passage through the envelope. But this I failed to do, in the present instance,—no spermatozoa were detected *in the interior* of the envelope, although many were easily observed on the *surface* at the point to which they had been applied by the pin. Yet, this egg, placed in a temperature of 66° FAHR., underwent segmentation in *three hours and thirty-two minutes*, and ultimately produced an embryo. A similar trial was made, at the same time, with an isolated egg after immersion for *two minutes* in water, which was then withdrawn, and the fecundating fluid applied as before. In this instance, several spermatozoa had penetrated for a short distance into the envelope, but had not reached the covering which immediately invests the yolk. No *respiratory chamber* was formed above the yolk in this egg, nor was any embryo afterwards produced. In a third instance, with an egg, which had been immersed for *five minutes*, the experiment was equally unsuccessful. On the 24th of March a further trial was made with an isolated egg, after *one minute's* immersion. The fecundating fluid employed had been obtained only sixteen minutes before it was used, and was applied by the head of a pin, once only, to one point of the egg. In this case, at the expiration of half an hour, I distinctly saw a single spermatozoon sticking by its larger extremity into the vitelline membrane, and a few minutes later there was evidence of the respiratory chamber being about to be formed. This egg underwent segmentation, and afterwards produced a good embryo. In a second egg immersed for *three minutes*, and in a third for *five minutes*, there were no appear-

ances of spermatozoa in the interior of the jelly, although they were in abundance at the point on the surface, to which they had been applied by the pin. Neither of these eggs underwent any change. But in a *fourth trial*, with an egg immersed for *ten minutes*, and to which the spermatozoa were applied in the same way as in the preceding, I saw, at the expiration of fifty minutes, four spermatozoa sticking in the vitellary membrane; soon after which the chamber was commenced, and at a later period segmentation of the yolk took place, and an embryo was produced. This was with fluid which had been obtained nearly an hour before it was employed. In a further trial, with an isolated egg, to which a full quantity of fluid was added, by three or four applications of the loaded pin's-head, after the egg had been *one minute* in the water, I detected several spermatozoa, within half an hour afterwards, sticking like the preceding into the vitelline membrane, and this egg also produced a good embryo.

It was thus evident, that in some cases, spermatozoa certainly penetrated through the envelope; but as there was also one instance, in which the egg had become fecundated, and afterwards produced an embryo, in which I had not seen spermatozoa within the envelopes, it was necessary to pursue the investigation further, to be quite assured of the fact of penetration as connected with the act of fecundation.

On the following day an opportunity occurred to me of examining the egg after impregnation by the natural union of the sexes. I had several pairs of frogs in a basin of water covered by a glass bell jar, when, having my attention directed for a few minutes to some other object, one pair of frogs spawned suddenly, as I knew by the sound of a plunge or splash in the water, which always occurs after the act, at the moment of separation of the sexes. Nearly the whole of the eggs were deposited in a mass, but there were a few which were detached from the rest, and were lying at the bottom of the water. This seemed a good opportunity to examine these eggs, which had been expelled naturally, singly, in search of the spermatozoa within the envelopes. *Fifteen* of these detached eggs were placed, each in separate cells, and carefully examined, during the first hour and a half after their expulsion. In the first eight or nine of these eggs, I was not able to detect even a single spermatozoon within their substance, and I began to look upon the previous observations, made by artificial impregnation, rather as the result of accident. In the tenth egg, however, I most distinctly saw spermatozoa in contact with the vitellary membrane, but in one or two eggs examined after this no spermatozoa were to be seen. Out of the fifteen eggs examined, there were only *two* in which I could detect spermatozoa. As each egg had been carefully marked, and a note made, as to whether or not any spermatid bodies had been detected within it; and as previous investigation had shown, that when any fecundation has been effected the yolk of the fecundated egg becomes depressed, and a chamber begins to be formed, in about one hour, or little more, after the spermatozoon is supplied to the egg, between its upper surface and the investing membrane,—there were ready means of learning, within a short time,

whether any, and which of these eggs had been fecundated. At the expiration of *one hour and twenty minutes*, the temperature being 54° FAHR., the chamber was beginning to be formed in the two eggs in which I had seen spermatozoa sticking in the vitellary membrane, and *these bodies were still readily detected in them*, and continued to be so for two or three hours afterwards. These two eggs ultimately produced embryos. But no change took place in any of the thirteen eggs, in which I could not detect spermatozoa,—no respiratory chamber was formed within, nor was any embryo produced. It was evident, therefore, that these had not been fecundated, and it is probable that the whole of the detached eggs were those which had last been ejected from the oviducts, possibly after the act of fecundation by the male had been completed. Having found spermatozoa in two only of these detached eggs, I then examined several of those from the mass. In each of them I found *numerous spermatozoa sticking around the yolk membrane*, and very many, in the clear space between the membrane and the granular middle portion of the envelope, which had not arrived at the membrane, and in *every instance* the eggs had been fecundated, as the chamber was being formed at the time of examination, about an *hour and a quarter* after the eggs had been deposited, so that the act of penetration by the spermatozoon through the envelopes as far as the vitellary membrane, seemed thus to be clearly established as connected with the act of fecundation. This was the case with all the eggs taken from the upper and middle portion of the mass. But in a very few eggs taken from the sides of the mass, I was not able to detect any spermatozoa, within the envelopes, or found only solitary instances of them. In these cases it was remarkable that no chamber had yet been formed above the yolk, although in some, in which spermatozoa were detected, it commenced at a later period, so that these appeared to confirm the deduction from experiment with reference to quantity of influence, or number of spermatozoa required to effect fecundation; while the penetration of the spermatozoa, as far as the vitellary membrane, and the subsequent development of the chamber above the yolk, appeared in the relation of *cause and consequence*, direct or indirect. I could not then observe, in the eggs thus examined, any penetration by the spermatozoa completely *through* the vitellary membrane into the substance of the yolk; although numerous spermatozoa were attached, by their larger ends, to every portion of the membrane, sticking out from it at right angles, with what, at first, appeared to be a knob or knot at the distal end, or rather as if that part of the spermatozoon had been shrivelled up or scorched. This appearance, as I subsequently found, was due to a loop, or distortion of the tail of the spermatozoon, consequent, apparently, on the death of this body. It was this looping of the tail which gave the appearance of a thick end to this part of the spermatid body first detected within the envelopes in a previous observation. All the spermatozoa seen in connexion with the vitellary membrane were perfectly motionless; so that when the looping of the tail has taken place the fecundatory influence of these bodies may be held to have been already exercised and exhausted.

At the expiration of three hours from the commencement of these observations, although the number of spermatozoa which had first been noticed within the coverings of the eggs submitted to examination appeared to have been diminished, there was still an abundance attached to the vitellary membrane. These appeared to afford a good opportunity for endeavouring to ascertain whether any part of the body of the spermatozoon is passed through the membrane into the cavity of the yolk. With this object in view, I placed a glass cell, which contained an egg, on one side, and waited for the rotation, or rather the gravitation, of the yolk within its envelopes. By this change of position of the yolk within its coverings—and which always takes place, if fecundation, or any change in the position of the entire egg, has been effected,—the flattened surface of the yolk, and the chamber above it, were brought beneath that part of the vitellary membrane into which spermatozoa appeared to have sunk deepest,—so that if any of these bodies had passed, or were in the act of passing, or had been partially protruded through the membrane into its cavity, which contained the yolk, it was fair to expect that they might thus have been detected. But not the slightest trace of penetration, even by a single spermatozoon, could then be observed*.

I may here remark, in anticipation of a future communication, that, previous to segmentation, the mass of the yolk does not appear to be invested by any distinct envelope within its so-called vitellary membrane, but the whole seems to be kept together by the natural coherence of the granules or cells of which it is composed; so that when the position of the egg is changed the yolk mass rotates within the vitellary envelope as a consequence of this change, owing probably to the excentric position, within its substance, of the progeny of the germinal vesicle†, contained in the

* Since this paper was communicated to the Society, I have succeeded, through the adoption of a different mode of examination, in detecting spermatozoa *within the vitelline cavity in direct communication with, and penetrating into the yolk*. They were first seen by myself, in company with a friend, on the 25th of March of the present year (1853) within the clear chamber above the yolk, at about forty minutes after fecundation, when the chamber begins to be formed. I have since repeatedly observed them within the chamber, and in some instances still in motion, in which state I have had opportunities of showing them to my friend Professor ELLIS of University College, and to two other medical friends, so that the presence of active spermatozoa *within the vitelline cavity* in the fecundated egg of the Frog may now be regarded as indisputable. The details of my investigation I reserve for a future communication, and will merely now add, that the spermatozoa do not reach the yolk of the Frog's egg by any special orifice or canal in the envelopes, but actually pierce the substance of the envelopes at any part with which they may happen to come into contact; as I have constantly observed while watching their entrance: sometime after they have entered the yolk chamber they become disintegrated, and are resolved into elementary granules. The importance of this fact of actual penetration by the spermatozoon into the yolk is indicated by WAGNER and LEUCKARDT, in their late Article, "Semen" (*loc. cit.* p. 507), in the following remark:—"The truth is, the 'how' of the fecundation is as far from our knowledge to-day as it was thousands of years ago; this process is still enveloped in what we feel inclined to consider 'its sacred mystery.' It would be different if we could prove that the spermatozoa really yielded the material foundation for the body of the embryo; that they penetrated into the ovum, and were developed into the animal (which was the assumption of LEBUEWENHOK, ANDRY, GUATIER), or else, that they became metamorphosed into the central parts of the nervous system."—G. N., April 18, 1853.

† Philosophical Transactions, Part I. 1851, p. 176.

middle of the dark hemisphere of the yelk, and which, possibly, may be of less specific gravity than the contents of the light-coloured hemisphere.

The facility with which the yelk can thus be made to rotate, and its flattened surface, and the clear space, or chamber above it,—commenced within one hour after the first encounter of spermatozoa with the egg,—can be brought beneath any portion of the vitellary membrane into which spermatozoa have penetrated,—thus enabled me to observe, within a comparatively short period after fecundation, whether any of those bodies are at that time in the act of passing through it. But although no spermatozoa could then be detected within the vitellary cavity, it must be borne in mind that these observations were made after the commencement of the formation of the clear space, or *respiratory chamber*, the consequence of a shrinking and depression of the surface of the dark hemisphere of the yelk as the *result* of impregnation, so that these negative observations did not afford any *proof* that penetration by the spermatozoon had not previously taken place. Indeed, all circumstances considered, there seems reason to believe that impregnation is effected very quickly after the encounter of the spermatozoon with the egg, as appeared to be shown in my former experiments with solutions of potass*. It is probable that it is *commenced*, as then suggested, *within a few seconds, or at most a very few minutes*, after such encounter; as in one instance of artificial impregnation in some observations made subsequently to those above detailed, I detected a spermatozoon in contact with the vitellary membrane, *within one minute* after the impregnating bodies had been supplied to an egg beneath the microscope. Certainly I believe that it is commenced, and probably completed, *within the first half-hour*; because, after that lapse of time, I have not yet been able to detect any change of place or position of the spermatozoa which have passed into the envelopes of the egg; whether they may have already arrived at, and become partially imbedded in the vitellary membrane, or whether they are still contained in the clear substance of the outer coverings, their force of penetration being exhausted before arrival at the membrane. After the lapse of half an hour, and frequently of a much shorter time, the caudal portion of the body of a spermatozoon which has penetrated into the envelopes becomes looped on itself, and this is a certain indication of the death of the object.

As no spermatozoa were seen, in these observations†, within the vitellary cavity at the early period above stated, it was not to be expected that they were likely to be observed at a later; although the eggs which had been the subject of these investigations, and which had been fecundated naturally, were continued to be watched until segmentation of the yelks commenced. This took place in the two eggs, in which alone, out of the fifteen first examined, spermatozoa were detected,—at the expiration of *five hours and twenty minutes*. But this was much earlier than in the great mass of eggs, of which these were a part; and was due to their having been exposed to a higher temperature, owing to heat radiated from my own body during their

* Philosophical Transactions, 1851.

† See the preceding note.

constant observation beneath the microscope,—rather than to that to which the undisturbed mass of eggs was exposed. The temperature of the atmosphere of the room at the time the eggs were deposited was 54° FAHR., but it gradually sunk to 51°·5 FAHR., and the water in which the eggs were contained to 50° FAHR. at the time when segmentation commenced, at the end of *six hours and thirty-five minutes*. In the most submerged it did not take place until *six hours and forty-five minutes* had elapsed.

In nearly the whole of the eggs of this mass which had been fecundated by the natural union of the sexes, I was struck with the fact that the quantity of spermatozoa which had penetrated into every part of their envelopes was very considerable. Very many of these had arrived at, and were sticking by their larger end into the vitellary membrane, from which they projected like spines from the head of a thistle; while there were many others which had not arrived at this part, their power of penetration being exhausted, and their progress inwards being arrested before they had passed more than half-way through the outer gelatinous coverings. Others, again, had penetrated to scarcely more than their own length into them, while a still greater number were simply in contact with, and adhering to the external surface.

When segmentation took place in these fully impregnated eggs, their yelks seemed to contract more powerfully, and the clear space, or respiratory chamber, formed in each became much larger than in others from the same mass in which but a few spermatozoa were detected; so that the inference deducible from the fact seemed to be, that a *plurality* of spermatozoa is necessary for the full impregnation of the egg, and the production of the robust and healthy embryo—although simple fecundation may result from the influence of only a very few spermatozoa, especially when such influence is aided by a considerable increase in the temperature of the surrounding medium.

Mode of Penetration by the Spermatozoon.—The preceding observations on naturally fecundated eggs were so decisive of the fact of penetration by the spermatozoon into at least the envelopes of the egg, and of the arrival at, and partial imbedment of these bodies in the vitellary membrane, that it seemed desirable to make further observations, by the artificial method, with a view to ascertain more directly the mode and circumstances of their entry; and these it was hoped might be learned through the facility with which the egg may be impregnated by direct application of spermatozoa to almost any part of its surface. Accordingly, on the following day, I placed an egg in a glass cell, beneath the microscope, and quickly afterwards applied to one side of it, by means of a pin-head, a quantity of spermatic fluid, obtained from the male only a few minutes before, and immediately filled the cell with water, and commenced the observation. The fluid was applied four times to the same part of the egg, the pin-head being loaded for each application, in order that a full sufficiency of spermatozoa might be furnished, before the water was added. The temperature of the room at the time of the experiment had been intentionally raised

to 65° FAHR., to afford a greater chance of success; as all my previous experiments had shown that more eggs became fecundated at a moderately elevated, than, under otherwise similar circumstances, at a low temperature. The instant the object-glass (the half-inch of Ross's microscope) was brought into focus, a vast quantity of spermatozoa were seen adhering to the surface of the egg at the part to which the pin's head had been applied. Many of them, attached laterally to the surface, had already ceased to move; others, only partially attached by their larger end, still vibrated the caudal or ciliated extremity rapidly, but did not appear to penetrate; while others, more centripetally attached by their body portion, vibrated the free extremity rapidly, and were seen in the act of gradually penetrating into the substance of the envelopes. I distinctly recognised one of these bodies which had just entered the envelope to a depth equal to about twice its own length, and when first seen had not reached so far as the middle or granulous layer of the envelope. Its motion was then slightly serpentine, and its course from without inwards was in a perfectly centripetal direction, with its thicker or body portion extended forwards, and in a line with the centre of the yolk, its progress inwards, as seen beneath the microscope, being as continued and as direct as that of an arrow. I kept this object in focus for several seconds, and watched it through the granulous layer, but ultimately lost it in the more dense and, as yet, unexpanded portions of the inner layers of the envelope, after it had been distinctly seen by a friend, who was with me at the time of the observation. A few seconds afterwards, as the envelopes became more expanded, very many of these spermatic bodies were seen to have already arrived at, and be in the act of passing through the inner or laminated portion of the envelopes,—the portion which, when the envelopes have acquired their full distension, by the imbibition of water, is seen to be that which immediately covers the vitellary membrane. A few seconds later a great abundance of them were seen in contact with the vitellary membrane itself; and some were even partially imbedded by their thicker extremity in its substance, and some of these showed an appearance as if they were actually penetrating through it. But in no one instance could I then satisfy myself that they did really pass through; since by alternately elevating and depressing the lens, and carefully noticing when the margin of the vitellary membrane was most distinctly defined, the appearances of perforation which some of them showed, seemed to be due to the spermatic bodies being imbedded in the membrane at some inclination to the plane of observation and to that of their direction being not quite centripetal. Yet there were other circumstances which seemed to show a likelihood that some spermatozoa do *actually pass through the membrane*. Thus, within the first few minutes after the impregnating fluid had been supplied to the egg under observation, and at the time when the spermatozoa, which had penetrated its envelopes, were first seen to have arrived at, and begun to enter the laminated portion, or *zona pellucida*, some of them were noticed to pass gradually onwards for a time, and then suddenly to disappear in an instant; as if, having passed into this tissue, they had also escaped

through the vitelline membrane which it covers. This mode of disappearance certainly is that by which the spermatozoa might be supposed to penetrate to the yelk, if in reality they do so*. Thus, presuming the body, or thicker portion of the spermatozoon, to perforate and pass through the vitelline membrane, the more slender or tail portion would follow quickly. But if this occurs with some, then it would be fair to expect that it would happen to the whole which arrive at and become partially imbedded in the membrane; especially to those which have sunk into it to a depth equal to one-half the length of the thicker or body portion; or, further, that the progress of others might be arrested before they had completely passed into the vitelline cavity; and, consequently, that some would occasionally be seen protruding into the interior. But I was not able, in the instance of the observations now detailed, nor in others afterwards made, to prove either of these suggested conditions.

In the observation now referred to, as well as in others since made, there were many spermatozoa which remained distinctly visible for several hours, in the same place, and in almost precisely the same position, sticking into the vitelline membrane, and retaining, at the end of a lengthened period, the same appearance as at first, excepting only that they seemed to have become smaller in diameter and to have their caudal portion more looped. In the present instance, at the high temperature of 65° FAHR. to 66° FAHR., not only were they distinctly seen at the commencement of segmentation of the yelk, which happened at the end of *three hours and twenty-two minutes*, but many of them were present until after the yelk had undergone several of its subsequent divisions. This was the case not only with those which had arrived at the vitelline membrane, but also with others which had never reached it, and had not penetrated further than to about the middle, or granulous portion of the envelopes; as happened with many spermatozoa, both in the eggs which were fecundated naturally, as well as in those which were the subjects of experiment, and were artificially affected. It was those which remained in the substance of the envelopes which usually disappeared earliest, becoming at first gradually fainter, and then more undefined in outline. This change was supposed to be due to a gradual diffuence of the substance of the spermatozoon, through the influence of the water imbibed by the envelopes; but whether this happened as part of the fecundatory process, or whether it was simply the natural process of decay, as other circumstances to be mentioned seemed to intimate, there was no distinct proof. It was remarkable, however, with reference to the *act* of fecundation, that in almost every instance, even of those spermatozoa which never arrived at the vitelline membrane, *the body portion was always directed towards the yelk*, usually peripherally, but sometimes inclined at slight angles to one side or the other; thus showing that it is invariably the body portion which penetrates.

* That this is really the fact, and that the actual penetration by the spermatozoon into the yelk chamber was observed on this occasion, is now rendered almost certain by my recent observations stated in the preceding note, p. 271.—G. N., April 18, 1853.



It may be matter of surprise, that, considering the immense quantity of spermatozoa which exist even in a microscopic drop of fluid, and considering also the abundance which come in contact with the surface of the egg, even when but a small quantity of fluid is employed, as in artificial impregnation, a much greater number do not penetrate than are usually observed to do so. There are conditions and circumstances which affect this result. Thus I have found, in repeated observations, that only those spermatozoa which, at the moment of first contact with the egg-envelopes, are in rapid action, and have their body portion directed, either perfectly centripetally towards the yolk, or at angles but slightly inclined to it, do by any possibility enter; while those which happen to be directed horizontally to the surface of the egg at the instant of contact, always adhere to it laterally, and lose their power of motion, but do not penetrate; and the like also is the case with those which become attached by their caudal end, and even with many which adhere by their thicker end, when they come into contact with the egg at very acute angles. Further, I have noticed, that a relatively much greater number of spermatozoa penetrate the envelopes when supplied to the egg immediately after this has been removed from the female into water;—especially when the spermatric fluid also has been recently passed from the male;—and more decidedly so when passed from a male in full season, at which time the movements of the spermatozoa are most energetic. Thus the chances of penetration through the envelopes, and consequently of fecundation of the egg by the spermatozoon, are in direct relation to these circumstances; and inversely to those of an opposite character, being less in proportion to the length of time the egg has been removed from the female, or the fluid from the male; the healthfulness of the parent, and consequent power of motion in the spermatozoon;—the temperature of the season, and the quantity supplied to the egg.

These were the conclusions deduced from the previous observations, and they have been fully borne out by subsequent experiments, some of which have been made in the presence of my friends Professors BELL, BOWMAN, CARPENTER, and Mr. BUSK, Fellows of the Royal Society, and Professor ELLIS, who permit me to mention the circumstance.

Narcotization of the Spermatozoon.—I may now mention some experiments which were made with the view to test the fecundatory influence of recently obtained spermatozoa when narcotized by chloroform. These experiments were suggested by a communication made to me by Mr. BUSK, F.R.S., who, after witnessing my mode of procuring the eggs of the Frog, conceived that a similar result might be attained by narcotizing the gravid animal without killing it, as is necessarily done in my experiment; and on putting this opinion to the test he found that it may be accomplished with ease and success. It then occurred to him to try the effect of exposing the spermatozoa to the vapour of chloroform, by simply covering the spermatric fluid contained in a watch-glass covered with blotting-paper wetted with the liquid,

and afterwards to apply the narcotized bodies to some eggs in the way done by myself.

The facility with which the spermatozoa can be narcotized by the mode now mentioned has enabled me to test the relation of their power of motion to that of their fecundatory property, and although my results differ somewhat from those obtained by Mr. BUSK, it may be well, perhaps, to relate them.

MM. PREVOST and DUMAS found, in addition to their many other excellent results, that spermatozoa are rendered motionless by an electric shock, and that then they do not impregnate the egg. They also found that opium and strychnine have a similar paralysing effect on these bodies. But it has since been suggested that the latter agents act on these bodies only in so far as they affect the chemical composition of their substance, and that the operation of electricity on them also is similar. But it yet remains to be shown whether any chemical change is produced in the substance of the spermatozoon, when simply narcotized by the vapour of chloroform, and not mixed with it in the fluid state; and when, although the power of motion is arrested, the vitality of the body is not destroyed.

Three eggs were placed in separate cells, and the spermatic fluid, immediately it had been obtained, was applied, *once* only to each, by means of the pin's head. These trials with the fluid in its natural state, mixed only with a small quantity of water, were made for the purpose of comparing their results, with those of others to be made with portions of the same fluid after it had been narcotized, and applied to eggs in this state at different periods. *One minute* after the application of the fluid I found an abundance of spermatozoa on the surface of each egg at the part touched, and some spermatozoa had not only already penetrated into the envelopes, but had arrived at, and were in contact with the vitelline membrane, or rather the *zona pel-lucida*. The respiratory chamber was afterwards formed above the yelk in each of these eggs, and two of these subsequently formed embryos; the third was only partially fecundated.

The spermatic fluid was then exposed to the influence of chloroform, in the way mentioned, about fifteen minutes after it had been obtained; and after seven minutes' exposure to it, and when the majority of the spermatozoa it contained had become narcotized, was employed in experiments. The signs of full narcotization are the entire cessation of all motion in the spermatozoon, which lies with its body extended at length, and not looped on itself. In the latter condition it is usually dead.

Three eggs placed in separate cells were then supplied with the narcotized spermatozoa applied to each egg three times by means of the pin's head. On examining the eggs six minutes afterwards, I was unable to detect even a single spermatozoon within the envelopes of either of them, either in contact with the vitelline membrane or in any part of the substance of their envelopes; although there was a great abundance of perfectly motionless spermatozoa on the surface. No chamber was formed above the yelk in either of them, nor did either of them produce an embryo.

Twenty minutes after narcotization, and during which time not a single spermatozoon had been observed in motion, a portion of them were applied in the same way as above to *three* other eggs, but not one of these became fecundated, no chamber being formed within them, and no embryo was produced. Many of the spermatozoa at this time were dead, as was shown by the looping of their bodies, but there was still a quantity which had been simply narcotized and lay extended at length.

One hour after narcotization another portion was applied to *two* eggs, one of which became partially fecundated. The chamber appeared in it above the yelk, the yelk underwent segmentation, and afterwards an embryo was begun to be formed, but its development was not completed, and the egg then perished. The second was not fecundated. The circumstance of this egg having been fertilized at so long a period after the fluid had been narcotized, leads to the conclusion that impregnation had been effected by *revived spermatozoa*.

On the following day I repeated these experiments with very mature fluid immediately after it was obtained, and in which the spermatozoa were exceedingly vigorous. *One minute and a half* after it had been exposed to chloroform, I found that the same effect had been produced on the spermatozoa as that which is produced in them by their admixture with very weak solution of potass; their power of motion was greatly increased, and the whole were in a state of intense action. After a few minutes' longer exposure their motion was perceptibly diminished, and at the end of about ten minutes it had almost entirely ceased, as only a few of them were then observed to move.

Three eggs were supplied, by means of the pin's head, with these narcotized spermatozoa. The respiratory chamber was formed in two of these eggs, and one of them underwent segmentation, and at the end of the fifth day an embryo had begun to be produced, but its development did not proceed, and this egg like the other two perished.

Six eggs were then supplied copiously by means of the loaded pin's head, three times applied to each, nearly the whole of the spermatozoa employed being then motionless, and having already remained so for about *ten minutes*, only an occasional one being observed in feeble action. Two of these eggs became partially impregnated, the usual chamber being formed in them, but neither of them underwent segmentation, nor was any embryo formed either in these or the others.

Three eggs were then supplied as above after the spermatozoa had been motionless during *twenty minutes*, not one being observed with the slightest action at the time of the experiment. No chamber was formed in either of these, but the whole remained unimpregnated.

Three eggs were the subject of further trial, when the spermatozoa employed had been motionless during *half an hour*. At this time one or two spermatozoa were again observed to be in feeble action, but no impregnation was effected.

Thirteen eggs were employed when the spermatozoa had remained motionless

during *one hour*, and when there was reason to believe that the majority of them had perished, but when there were still a very few among them in motion, having either revived, or but recently escaped from spermatozoal cells contained in the fluid. No impregnation took place in either of these instances.

Thus the results of these experiments appear to show that the spermatozoon does not impregnate *when entirely deprived of its power of motion by narcotization*, and disenabled to penetrate into the envelopes of the egg; and, consequently, that its fecundatory power has a close relation with its motion, or force of vitality. In this, then, they coincide with those of the previously detailed experiments, and go with them to show that the *act of fecundation of the egg of the Frog*, and probably also of all the Vertebrata, is the result of a power in the spermatozoon, which, in its operative condition, is characterized *by motion*; and that this power is totally independent of everything approaching to volitional influence, in the impregnating body, but seems to be in direct relation to physical causes.

MM. PÆRVOST and DUMAS appear to have thought that the spermatozoa which they observed within the egg-envelopes had entered through means of the infiltration, or imbibition of water by the envelopes; and the gist of their experiments was to show that particles of solid matter *do enter* during such infiltration, or, as it has since been designated, *endosmosis*. But some experiments, elsewhere detailed*, have led me to believe, that only such solid particles as are very much smaller in diameter than the spermatozoon can so enter with the water by endosmosis; while the circumstances now detailed fully prove that the entrance of the spermatozoon is not the result of simple infiltration, but is that of the operation of direct mechanical or physical power in that body. Thus, if it were mainly dependent on endosmosis, the perfectly motionless spermatozoon would enter the tissues and fecundate the egg equally well with the active, but this, as we have seen, is not the case, and this was shown by the physiologists now referred to. On the other hand, the circumstance that the spermatozoon invariably enters the tissues with its thicker or body portion directed forwards, and sometimes even at an angle slightly inclined to the centre of the yelk, and always impelled by an oscillatory or vibratile motion of its caudal portion, seems to show that its power of penetration is not only a necessary condition of its function, but is inherent as such in its organic composition. It may yet be true, nevertheless, that the passing of the spermatozoon through the substance of the envelopes to the vitellary membrane may be indirectly *aided* by the endosmic action of the envelopes. But this aid does not consist of any imbibing property in the tissues. It appears to be simply of a negative character, and to be the result of a decrease of density in the tissues, which, through their imbibition of water, and consequent expansion, are caused to offer less and less resistance to the propulsive force of the spermatozoon. This I believe is the proper explanation of the fact of penetration by this body.

But although penetration be not caused by the endosmic action of the envelopes,

* Philosophical Transactions, 1851, p. 224.

it may yet be thought to be induced by some attractive power in the substance of the yolk itself; and that, therefore, the entrance of the spermatozoon may be as much due to the egg as to any power of motion in the penetrating body. An accident has enabled me to test the validity of this surmise.

I had promised to show the fact of penetration to a friend, but circumstances prevented me from doing so until the season had nearly passed, and the whole of my frogs had spawned. I determined therefore, as a last resource, to endeavour to obtain some fecundatory fluid from a male which had already paired two or three days previously, and to employ it with the only eggs I had then left, which remained in the body of a frog that had been killed *twenty-six hours* before, and which, as former experiments had shown, it was probable had lost their vitality. The fluid required was obtained with ease, but mixed with a large quantity of spermatozoal cells. This was supplied to some eggs from the dead frog, since, although I did not expect that the eggs would be fecundated, I hoped for an opportunity of again witnessing the penetration by the spermatozoon. The spermatozoa in the fluid obtained were very active, and fully efficient, and were supplied in abundance to several eggs in separate cells under the microscope. The envelopes of the eggs expanded as usual, and endosmosis went on in a perfectly natural way, and an abundance of spermatozoa adhered to their surface. At the expiration of from fourteen to twenty minutes I found that several spermatozoa had penetrated the envelopes and were adhering in the usual way to the vitelline membrane. In one egg there were six, in another five, and in a third four, distinctly visible in the plane of observation that could be brought at once within view with the microscope, besides others recognisable on changing the focus. It was thus evident that spermatozoa, even of the previously paired Frog, still retained their *power of penetrating into dead eggs*, as these ultimately proved to be, after careful preservation to the sixth day in a favourable temperature. The spermatozoa were distinctly visible within the envelopes, without change of position, for several hours, but no fecundation was effected by them; no chamber was formed in either of the eggs, no segmentation took place, nor was any embryo produced. These circumstances seem to show that the eggs were already dead, as was supposed, before contact of the spermatozoon; consequently that the entrance of the spermatozoon into the envelope is due to a power inherent in the penetrating body, and not simply to an attraction on the part of the yolk; although from the fact that the spermatozoa usually enter in a centripetal direction, it is probable that some influence may be exerted by the yolk or its vesicle, although penetration is mainly the result of force in the spermatozoon.

The facts now stated of penetration by the spermatozoon seem to lead us better to understand the nature of some experiments with solutions of caustic potass, which are detailed in my former paper. I have repeated these experiments, during the past season, in the presence of several friends, Professors SHARPEY, ELLIS, and BELL, and Messrs. BUSK, TOMES and WATERHOUSE, with results precisely similar to those which

are detailed in that paper; viz. that when diluted spermatic fluid, recently obtained, is applied to a set of eggs, and—as soon afterwards as the experiment can be made, a solution of potass,—of such a strength as is known by previous microscopic observation to have the property of instantly decomposing the spermatic body,—the solution being washed away quickly after its application, by repeated quantities of water,—to prevent its affecting the egg itself—that then—in some instances—even when the interval of time between the application of the spermatic fluid and the subsequent application of the potass does not exceed a few seconds,—impregnation of the egg is effected; as is proved by the formation of the chamber, the segmentation of the yolk, and perhaps the formation of an embryo. Further, that, all circumstances being similar, excepting only that the interval of time between the application of the spermatic fluid, and, subsequently, that of the solution of potass be prolonged,—the production of an embryo is not only more certain to take place, but the number of embryos produced is increased. The now ascertained fact of *almost instantaneous penetration* by the spermatozoon, which, as before shown, sometimes arrives at the vitelline membrane in less than *one minute* after its application to the egg, confirms the principal conclusion deduced from the potass experiments at the time they were made, viz. that *impregnation is commenced at the instant the spermatozoon is in contact with the egg*; while it also seems to afford the true explanation of the nature of those experiments, in which it may now be presumed that some spermatozoa had actually penetrated into the substance of the envelopes before the application of the solution of potass, and thus had already passed out of the reach of its destructive influence, the effect of which on the egg itself was obviated by speedy dilution and ablution with water.

The potass experiments may thus be regarded as confirming by anticipation the results now obtained by direct observation with the microscope, with respect to the rapidity of operation by the spermatozoon; and they seem also to support the view of the essentiality of the motor power of this body to its functional action. A similar view may be taken, as indeed was held at the time, of the nature of the Carmine, the Gum, and the Starch experiments, that the operation of these substances in preventing the fecundation of the egg is *entirely mechanical*, and that they do so simply by offering a mechanical impediment to the spermatozoon, a conclusion which seems to be fully supported in the present inquiry.

14. NATURE OF THE INFLUENCE OF THE SPERMATOZOON.

Having seen in the preceding experiments and observations that fecundation of the egg is effected by the spermatozoon only while this body retains and continues to give evidence of its vitality in its power of motion, and that its vitality either may be destroyed, or its operation be for a time entirely arrested by electricity and by chloroform,—the question naturally arises—in what way, then, is its fecundatory influence to be explained? Is it *simply* by diffidence of the substance of the spermatozoon, and the *chemical fusion* or combination of this with the contents of the egg, after the sper-

matooon has penetrated through the envelopes and arrived at the vitelline membrane, or the yolk? Or is it by its expenditure on the yolk, of a *force* or power of vitality, which is inherent in the body of the spermatozoon? Or is it by the joint cooperation of both these conditions—the expenditure of a *force* with diffuence of material substance? I have endeavoured to put the first of these questions to the test of experiment;—although, it must be remarked, that the experiments made seem to be open to some objections, which at present cannot be fully answered. Yet knowing what we do of the disposition and great readiness of all matter to enter into new combinations, and affect, or entirely change the condition of the whole body operated upon,—as,—not to mention the effect of the poison of serpents, or the introduction of dead matter into the living, in dissection,—can be shown in the effect, on the yolk of the Frog's egg, of extremely minute quantities of chemical compounds diffused in water,—there has seemed fair reason to think that the question thus started may be examined by experiment. The rapid endosmic action of the envelopes of the egg at the instant of their contact with any aqueous fluid, and the rapidity with which the yolk itself then becomes affected, seemed to favour the idea that if the bodies of the newly obtained spermatozoa could be quickly reduced to a state of diffuence simply by mechanical means, without the addition of any menstruum, saving only a very small quantity of water, and be applied in this state of diffuence to the egg at the instant after it has left the body of the female,—that then an experiment thus made would be a fair test of the question. Some imbibition of the substance of the spermatozoon with the water might thus be expected to take place, and some changes in the egg to follow, if impregnation be the result of simple chemical combination of the substance of the male with that of the female. The presence in the experiment of a portion of water holding the diffuent spermatic substance in suspension, appeared at first sight to be an objection, but this seemed to be met by the fact that water *must* always be present to ensure the natural fecundation of the Frog's egg; and that it not only permeates the envelopes, and is the means of facilitating the entrance of the spermatozoon, but that it passes even to the substance of the yolk itself, as shown in the experiments with large quantities of potass before alluded to, and also in others made by immersion of eggs in water with given proportions of potass in solution. As the details of these latter experiments are contained in a paper which is now in the Archives of the Royal Society*, I need give but little more than the results of these experiments, in the present communication, before proceeding to relate those now referred to with the diffuent spermatozoa.

Immersion of Ova in Solutions of Potass and Soda.—These experiments were commenced in April 1850, before the publication of some which were made with solutions of caustic potass, by M. QUATREFAGES, on the spermatozoa of one of the marine worms, *Hermella*, and read to the Institute on the 24th of June 1850†.

* MS., No. 762, p. 13 to 26; also Proceedings, vol. vi. p. 83.

† Comptes Rendus, June 24, 1850, and Annales des Sciences Naturelles, 3^{me} série, tom. xiii. in Nos. marked "February" and "March 1850."

The object which I had in view was to endeavour to learn what proportion of the carbonates of potass and soda may be present in a given quantity of water without destroying the fertility of the egg. The first trials with potass were made with eggs which had already been fecundated, and were undergoing segmentation. It was then found that when fecundated eggs, arrived at the second stage of segmentation of the yelk, were immersed in a solution, composed of *twenty grains* of the fused carbonate of potass to one ounce of water, the yelks became shrivelled, and decomposition was commenced within *three minutes* after immersion; thus proving that even when the envelopes of the egg are almost fully expanded, and, consequently, when their endosmic action may be expected to have become greatly diminished, that endosmosis may still be going on with much energy. It is right to mention, however, that in this and the following experiments, the endosmic action may have been, and probably was, increased by the removal of the eggs—which already retained water in their tissues—to the more dense fluid solution; but as the experiments are comparative, the circumstance is not of much importance.

On the contrary, when some eggs were immersed in a solution of *one-fourth* of a grain of the salt to one ounce of water, and the potass in admixture with the water was thus reduced to $\frac{1}{1330}$ th part of the whole, development went on more quickly than in other eggs of the same brood, impregnated at the same time, but which were placed in pure water apparently through the potass occasioning a slight increase of temperature.

These results appear to be explicable only on the assumption that the weak potass solution acts as a chemical stimulus,—perhaps both to the egg and to the spermatozoon,—at the time of fecundation, a view which derives some support, in so far as refers to the spermatozoon, from direct observation with the microscope. Thus, when a drop of the *two-grain* solution of potass is applied to spermatozoa on a plate of glass, covered with talc, beneath the microscope, the movements of these bodies are not instantly arrested, as by the stronger solutions, but gradually become slower and slower, until they entirely cease, apparently, in proportion as their substance becomes affected by the potass. The *one-grain* solution very much accelerates the movements of these bodies during the first few seconds; but in a few seconds longer these also become diminished like the preceding. But the effect of the *half-grain* solution, although exciting the spermatc bodies to activity as soon as it comes into contact with them, continues its effect for a much longer time without acting injuriously upon their substance; and it is not until after the lapse of many minutes that the intensity of the motion excited by it is observed to be diminished, and the movement to slacken and afterwards gradually cease.

With regard to the carbonates of soda, I may state that the general effect of these on the impregnated and on the unimpregnated egg are very similar to those of potass, but are far less prejudicial, or marked, so that it is unnecessary to detail the experiments.

The general conclusions deducible from these observations, with reference to the nature of the influence of the spermatozoon in impregnation, seemed to be, that as water which holds in suspension or solution any chemical substance, conveys some portion of that substance at all times, with great rapidity, to the immediate vicinity of the yelk itself, which becomes affected by it, and especially so at the time of its first coming into contact with the envelopes of the newly deposited egg,—the substance of the spermatozoon also, if diffused in water, might be so conveyed, in the experiments proposed. In addition to this conclusion, there are others, which, although less directly connected with the object in view, are not less important. Thus it appears that an alkaline fluid, in large quantity, is as injurious to the vitality and fecundatory influence of the spermatozoon as to the fertility of the egg.

Trituration Experiments.—The endeavour in these experiments was to put to the test the question proposed in the preceding observations,—whether, if the spermatozoon be reduced to a state of diffuence in water quickly after it has left the body of the Frog, and before it can reasonably be supposed that any change in the chemical constituents of its body has taken place, the egg can be fecundated through simple imbibition of the substance of the spermatozoon, conveyed to the vicinity of the yelk with the water during endosmosis, at the time when endosmosis of the envelopes of the egg is most energetic?

Although it had been constantly found, both by the authorities before cited, and since by myself, that when the spermatozoon has ceased to move, and is believed to be dead, no impregnation results from the contact of its motionless body with the egg; yet there still appeared to remain some doubt as to what this want of operation is then due; whether it is to be attributed to the organic death and incipient decomposition of the body, or whether to the loss or suspension of some power which is characterized by *motion*, and through which its function is exercised?

The following experiments, with reference to this question, were commenced before I was aware of the fact that the spermatozoon penetrates into the envelopes of the egg; a fact which, when known, seemed further to necessitate the inquiry, as probably tending to show whether impregnation is merely the result of a union of the substance of the spermatozoon with that of the yelk or *its vesicle*; or whether it be not primarily due to the transmission of some dynamic influence, force, or peculiar vitalizing power from the spermatozoon to the egg at the time of, or previous to any fusion with it of its material substance.

The first experiments were made by breaking down the bodies of the spermatozoa mixed with water, and, after filtration, applying the filtered liquid directly to the egg; the following being the method pursued:—

The bottom and sides of a very small glass mortar, and the glass pestle employed with it were ground together with fine sand and water, for the purpose of slightly roughening their surfaces, and of ensuring their contact at every part, and thus to render the crushing of the spermatozoa more certain.

The spermatic fluid first employed was procured from a Frog early in the season, and consequently contained a large quantity of spermatozoal cells from which the spermatic bodies had not been liberated. A difficulty immediately occurred which had not been anticipated. The fluid was divided into two parts, one of which was triturated gently with the pestle and mortar for two minutes, and was then examined with the microscope. It still contained an abundance of active spermatozoa, and many cells. The trituration was continued for three or four minutes longer, without any marked result, as active spermatozoa were still abundant in the specimens of fluid examined. It was then repeated for several minutes with increased rapidity and force, when the fluid suddenly became partially coagulated, and separated into opaque white flocculi, composed almost entirely of cells and granules, and a transparent fluid in which there were scarcely any granules. It was then found that nearly the whole of the spermatozoa had been destroyed, as I was not able to detect even one in motion. The thinner portion only of this fluid, when placed on the filter, passed through, and it did so with great slowness and difficulty.

These circumstances are stated to show the mode of proceeding, and the accidents to be guarded against. The conclusions deduced from these trials were, that the spermatic fluid was not fully matured, and probably contained albumen, which, it is stated*, does not exist in mature spermatozoa; also that the substance of the spermatozoa had undergone some chemical decomposition, perhaps from excess of heat evolved in the act of trituration, through too much force being employed; so that the results of these experiments could not be depended on. A further trial was therefore made a few days afterwards. The fluid obtained from a mature Frog was divided into two parts, one of which was reserved for simple artificial impregnation; while the other was triturated slowly and carefully in the mortar with but little more exertion than was necessary to keep the pestle in constant motion for about fifteen minutes; no water being added to the fluid. When placed on the filter it became necessary to add to it about an equal quantity of water to facilitate its filtration. Portions of the fluid which then passed through, holding the substance of the broken-down spermatozoa in solution, were caught in separate glass cells.

Ten eggs were passed into a cell about half filled with the filtered fluid, and the cell was afterwards filled up with water. No other change than a slight enlargement, after the lapse of some hours, occurred in these eggs; no chamber was formed, nor did any segmentation take place in either of them, consequently no embryos were produced.

Fifteen eggs were placed in a cell which contained about half the quantity of filtered fluid employed in the preceding; but no fecundation took place in either of these eggs.

Eleven eggs in a third cell with filtered fluid were equally unfruitful.

Ten eggs in a fourth cell gave similar results.

* Article "Semen," Cyclop. Anatomy and Physiology, vol. iv. p. 506.

One hundred and forty-seven eggs were then placed on the topmost of the filter papers, which had been employed in separating the fluid used in the preceding trials, and on which were the remains of the broken-down spermatozoa, with, probably, some which had not been injured. This was about four hours and a half after the spermatic fluid had been obtained, so that on this ground but little success could be expected. Not a single embryo was produced.

The portion of reserved fluid which had not been triturated was then applied to four eggs in separate cells, by means of the pin's head, when each of these eggs became fecundated, and not only underwent segmentation, but each afterwards produced an embryo. As this experiment was made for the purpose of comparison with the preceding, it is clear that the failure in the case of the mass of eggs on the filter paper was not due to the length of time the fluid had been obtained, but to the effect of trituration; and it seems equally clear that the substance of the broken-down spermatozoa dissolved in water and passed through the filter is as inefficient as the *liquor seminis* has heretofore been proved to be*.

These experiments were afterwards repeated, in the presence of Professors SHARPEY and ELLIS, with similar results. Usually there are still some spermatozoa which escape being crushed during trituration, and, being uninjured, remain on the filter paper capable of effecting impregnation when eggs are passed upon it in water, as was the case on the following occasion.

Seventy eggs were first added to some filtered fluid, which resulted from the trituration of spermatic fluid, and had passed through four filter papers, but not one egg became affected, not one embryo was afterwards produced from them; while of sixty-two eggs placed on the topmost filter paper in water, amidst the remains of broken-down spermatozoa, with many which were still active and uninjured, more than one-third of the eggs became fertilized, and twenty-three afterwards produced embryos.

It was thus evident that no impregnation results from an immersion of the egg in fluid which is derived from the broken-down body of the spermatozoon, mixed with water, and separated from the more solid parts by filtration. But as some objections may fairly be offered as to the validity of this experiment, it seemed desirable that trial should be made without having recourse to filtration, and without removing any portion of the destroyed spermatozoon; but, on the contrary, by applying the whole of the triturated fluid immediately to the egg before it is immersed in water; and at the same time to compare the result obtained with that of an experiment by simple artificial impregnation, with a sample of the same spermatic fluid which had not been triturated; the experiments in both cases being made at the same time, and with eggs from the same female, and placed under precisely similar conditions in regard to light, heat, &c. A comparison of the relative number of embryos which might result from these experiments, it was thought, would show, not merely whether

* Philosophical Transactions, 1851.

impregnation can or cannot be effected by the direct application of the diffident substance of the spermatozoon to the egg, but also whether the influence of the spermiatic body is essentially dynamic in its character.

The quantity of spermiatic fluid obtained for these experiments, mixed with a small proportion of water, measured about fifteen minims; and to this a further quantity of water was added, making the whole thirty minims. This was divided into three equal portions. The first portion (*a*) was triturated slowly and continuously for fifteen minutes, after which it was slightly turbid, and on examination with the microscope was found to contain a large quantity of granules, each of which did not exceed in size one half of the diameter of a spermatozoon. It also still contained a good abundance of active spermatozoa, although the quantity of these was greatly reduced, and the movements of those which remained were impeded by the quantity of granules. The second portion of fluid (*b*) was triturated with the addition to it of about one grain weight of well-washed sand, and after three or four minutes presented a turbid appearance. At the end of seven minutes, when examined by the microscope, it was found to contain a great abundance of organic granules, intermixed with the sand, and the number of spermatozoa had been greatly reduced; and three or four minutes later nearly the whole of the spermatozoa were destroyed. The following experiments were then made with eggs obtained from the female with which the male that supplied the fluid had been paired. The temperature of the atmosphere at the commencement of the experiments was 57° FAHR., and that of the water employed 55° FAHR. At the end of the experiment, the commencement of segmentation of the yolks, the atmosphere was 61° FAHR., and the water 59° FAHR.

One hundred and seventy-nine eggs were passed into a glass dish, and the triturated fluid (*a*) was immediately poured over them, and the dish quickly filled with water. The yolks of many of the eggs soon became irregular and contracted, and at the end of *four hours and thirty-one minutes* segmentation had commenced in some of them, and ultimately, *sixty-seven embryos* were produced; so that one hundred and twelve eggs were not fecundated.

One hundred and sixty-seven eggs were passed into a second dish, and the fluid (*b*) which had been triturated with sand was immediately added to them, and afterwards the dish filled with water as in the preceding. But *not a single egg* became fecundated, nor was a *single embryo* produced. It must be stated, however, that much of this result was afterwards found to have been due to the admixture of sand with the fluid acting mechanically, as I shall presently show.

Two hundred and thirty-nine eggs were also passed into a dish of the same dimensions as the preceding, and the third portion of fluid (*c*) reserved for simple artificial impregnation, and which had not been triturated, was supplied to them at, as nearly as possible, the same time as the triturated fluid to the preceding. This was at *thirty-five minutes* after it was obtained. Segmentation commenced in nearly the whole of these eggs in *four hours and fourteen minutes*, and at the end of the fifth day *two*

hundred and six embryos were produced, so that only *thirty-three* eggs failed; thus leading to the inference that the failure in the experiments (a) and (b) was owing to the spermatozoa being destroyed; and, consequently, that the application of the substance of the body of the spermatozoon to the egg is not alone sufficient to effect fecundation; so that fecundation cannot be regarded as the result of simple chemical combination of the substance of the spermatozoon with that of the egg, but, essentially, may be due to some dynamical influence in that body.

At the time of making these experiments I made also two others with a view to this hypothesis; and for the sake of correct comparison employed eggs from the same female, and placed them in a precisely similar condition with regard to light, heat, and the quantity of water employed. The fecundatory fluid, however, was not from a male in full season, as above, but was what may properly be regarded as *senile*, it being purposely obtained from some frogs which had been kept separate from the females, having already paired and spawned from five to ten days previous. It was slightly translucent, and contained, relatively, but few living spermatozoa, the majority of which were languid in their movements; but there were many which appeared perfectly dead and motionless, and there was a good proportion of spermatozoal cells. In the first experiment *one hundred and sixty-seven* eggs were supplied with a portion of this fluid, and segmentation commenced in some of them in *four hours and forty-two minutes*, or not so soon as in the experiment (c) by *twenty-eight minutes*, and afterwards only *forty-three* embryos were produced. In the second trial, made at the same time with *one hundred and seventy-three* eggs, segmentation took place in *four hours and forty-one minutes*, and *one hundred and twenty-six* embryos were produced, *forty-seven* eggs being unimpregnated. If the difference in the length of time which elapsed between the encounter of the spermatozoon with the egg and the commencement of segmentation in these two experiments, as compared with the preceding one, be not fairly referable to a less degree of vitalizing power in the spermatozoon in the latter, it seems difficult to understand to what other cause it can be assigned; seeing that the eggs employed were from the same female in each experiment, and that all the conditions were similar. It seems, as it were, *inversely*, to show, that the most important condition of the impregnating agent is its possession of some dynamical quality, the degree or intensity of which is expressed in that of its power of motion, and which, possibly, it may transmit from itself to the egg, during the act of fecundation.

These results sufficiently mark the injurious effect produced on the fecundatory fluid by trituration, whether with or without admixture of foreign substances; and while these seem to prove that attrition of the spermatid bodies with particles of solid matter, by lacerating and mechanically destroying them, is fatal to their function, other experiments showed that even their simple admixture with any finely comminuted solid materials greatly interferes with their operation, by the mechanical impediment which such materials oppose to their free motion, and to their penetra-

tion into the envelopes of the eggs, whether such admixture be made with the bodies themselves, or with the water in which the eggs are immersed before they are supplied to it.

The question proposed in these experiments by trituration we may now look upon as to some extent, if not entirely, answered; as it seems that although the spermatozoon be broken down mechanically before any chemical change can be supposed to have taken place in it,—indeed, at the very time it is giving evidence of its vitality,—and its organic substance so broken down be quickly afterwards brought into contact with the egg in water, at the moment when the egg is most susceptible of its influence, yet that no fecundation is then effected by it. It may be urged, it is true, that there was no direct *proof* that the spermatic substance was actually conveyed to the yolk by, or with, the water in these trials; but there was one circumstance which gave fair reason to believe that it really was so conveyed. The yolks of some of the eggs became much contracted, and, as it were, shrivelled, as when affected by potass solution; of the penetration of which to the yolk there is absolute proof in the decomposition of the egg which quickly succeeds to its introduction. It has also been shown in a preceding experiment (p. 241) that the yolk becomes similarly affected when decomposing spermatic fluid is applied to it.

The conclusion then which seems to me to be deducible from these investigations is, that fecundation is not simply the result of a mere fusion or chemical admixture of the substance of the spermatozoon with that of the egg; although such fusion, probably, is necessary to the production of the organized body of the embryo, in determining its structural and psychical peculiarities, and its definite species; and more or less of which may, possibly, help to determine the sex, and the extent to which the structural and psychical peculiarities of the male parent are transmitted to the offspring. That this fusion of some portion of the spermatic substance with the egg does actually take place, either when the spermatozoon has arrived at, and become imbedded in the vitelline membrane, as in the Frog; or, as stated by a recent observer*, to occur in the *Ascaris Mystax*, when it is in immediate contact with the yolk itself, is probable, from the considerations now adduced. These views are countenanced by the now established fact that the spermatozoon of the Frog, and probably also of other Vertebrata, does not fertilize the egg either when it is perfectly motionless,—whether from actual death, or from suspension of vitality by narcotization; nor even, as we now find, though living, while simply in contact with the surface; nor until after it has actually passed into the envelopes, and arrived at the immediate vicinity of the yolk,—facts which seem, I think, inferentially, to show a probability that, *whatever conjugation of materials may be effected, some vitalizing dynamic influence is also expended by the spermatozoon on the contents of the yolk in the production of its changes*,—phenomena which have not been found to take place

* Dr. NELSON, "On the Reproduction of the *Ascaris Mystax*," Proceedings of the Royal Society, vol. vi. p. 86. Philosophical Transactions, 1852.

on the simple application to the egg of the diffuent spermatic substance. In the absence, then, of all proof that simple fusion only of the substance of the spermatozoon with the contents of the egg is the chief condition of fecundation, we have, in the facts now referred to, much reason to regard fecundation as primarily dependent on the existence of a force or power in the spermatozoon, by which this body is enabled to arrive at the object to be fertilized, and which, visibly expressed in that of its power of motion, is lost quickly after it has penetrated into the yelk-membrane or yelk:—this, probably, is the instant of fecundation. May not the spermatozoon, then, be viewed as *the organ of a special form of force in the male body* for the production of these results, in the same way as the nervous structure is regarded as that of the nervous, and the muscular as that of muscular power? We have already seen that its function is exercised more or less readily and perfectly in relation to two conditions: first, that of its full maturity, in which its power of motion is most intense; and next, in relation to the influence of external physical agencies, which cause this power to be evolved through its organic composition, in a greater or less degree, whether the external influence be that of heat, or the operation of a chemical power, as in the instance of potass in the preceding experiments.

XI. *Description of some Species of the extinct Genus Nesodon, with Remarks on the primary Group (Toxodontia) of Hoofed Quadrupeds, to which that Genus is referable.* By Professor OWEN, F.R.S. &c.

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THE application of part of the Government Grant, allotted to procuring drawings of rare and nondescript fossil remains, has furnished the requisite illustrations of the following Memoir on some extinct Herbivorous animals, which indicate a primary group of the *Ungulata*, distinct from and probably equivalent to one of the great divisions called *Proboscidea*, *Artiodactyla*, and *Perissodactyla*.

Genus NESODON*, O.

The genus so called, from most of the enamel-folds penetrating the crown of the molars being, when exposed by use, of an insular form, was founded by me in 1846†, on portions of an upper and lower jaw, with teeth, transmitted by Captain SULLIVAN, R.N., from the S.W. coast of Patagonia.

I have since received from the same zealous contributor of materials towards the advancement of our knowledge of the ancient zoology of South America, more extensive portions of both upper and lower jaws with teeth of the generic character of *Nesodon*, but belonging to a smaller species than that of which the first described specimens formed part; the second collection also contained detached teeth, indicating much larger species than either of the two above indicated.

I propose to commence with the description of the remains of the smaller species, which from its size may be termed—

Nesodon ovinus, Plates XV. and XVI.

With the upper jaw of this species (figs. 1–5) are preserved the articular surface (figs. 1 and 3, *g*) for the right ramus of the lower jaw, the zygomatic arch (figs. 1 and 2, *z* and *z'*) and part of the nasal bones (figs. 1 and 2, *n*).

The total length of this portion of skull from the post-glenoid process‡ (figs. 1 and 3, *pg*) to the anterior incisor (fig. 1, *d i*) is 6 inches 4 lines. The breadth of the

* *νησος* insula, *δένος* dens.

† Abstracts of Papers in 'Reports of the British Association' for 1846, vol. xvi. p. 66.

‡ The process, sometimes much developed, as in the 'Rhinceros, *e.g.*, is so called in my Catalogue of the Osteology in the Mus. Coll. Chir., which answers to the part described as the "middle root of the zygoma" in Anthropotomy (Bourguery, *Traité Complet de l'Anatomie de l'Homme*, tom. 1^{re}, plate 23, fig. 10, *c*.)

glenoid cavity is 1 inch 4 lines. It is slightly concave from side to side, slightly convex from behind forwards, where it forms the 'eminencia articularis' (fig. 3, *g*), which is about 3 lines in antero-posterior extent at its middle, widening to twice that dimension at the outer and the inner ends of the eminence. The narrower concavity behind this part is bounded behind by the thick process (*pg*), extending a little below the level of the outer end of the eminencia articularis, convex anteriorly and below, concave behind, into which deep concavity a venous canal or sinus appears to have opened.

The sutural surface on the inner side of the squamosal (fig. 3, *w*), for junction with the alisphenoid and tympanic, extends from the eminencia articularis (*g*) to the lower end of the post-glenoid process (*pg*), behind which the broken surface of the back part of the squamosal shows part of a smooth cavity.

The zygoma (fig. 1, *z*, *w*) is deep and strong, bent from behind forwards in a gentle sigmoid curve, first convex upwards, then more deeply concave to form the lower part of the orbit, very slightly arched outwards, and formed by the squamosal (*w*) and malar (*z*) in almost equal shares, the major part of the suture between them describing a strong curve, convex forwards. The zygoma is thickest at its two extremities, and of least vertical extent near its anterior one, where it forms the orbit (fig. 1, *o*). There is no trace of post-orbital process from either the malar or squamosal parts of the zygoma, nor of any sutural surface for a descending post-orbital process, as in the Horse. The orbit communicates as extensively with the temporal fossa as in the Tapir and Rhinoceros. The maxillary bone (fig. 1, *m*) sends backwards a plate along the inner side of the fore-part of the zygoma, which unites by a squamous suture with the malar; the malar terminates forwards, gradually contracting as it ascends, at about the middle of the anterior boundary of the orbit, whence it extends but a very short way upon the face. The articular surface for the lacrymal bone may be seen upon the conjoined orbital portions of the malar and maxillary, but the lacrymal itself is lost: its extent upon the face must have been as trifling as that in the Hyrax. The facial part of the maxillary is lofty but not long, gradually decreasing in depth to its anterior border, which is nearly vertical, measuring 1 inch 4 lines in that extent, and wholly united to the premaxillary. The outer facial surface of the subquadrate facial plate of the maxillary is concave, and perforated by the antorbital canal (*s*), which opens 6 lines in advance of the orbital border of the maxillary, and 13 lines from the suture with the premaxillary. There is a second small foramen in advance of the oblique rough suture (ib. *m*) for the palatine. The palatines are lost in the specimen; they appear to have formed the middle two-fourths of the produced back part of the bony palate.

The extent of the suture of one maxillary with the opposite maxillary is 2 inches, and behind this the extent of the suture of the maxillary with the palatine is 1 inch: at this part the bone gains much in depth. The palatal plate of the maxillary slopes from the alveolar border towards the median suture, with a curve concave downwards, giving an arched form to the bony roof of the mouth at its anterior two-thirds, behind which the bony palate is less concave transversely, and is convex longitudinally. The post-palatal canal opens obliquely forwards by a foramen situated a little in advance of the middle of the palatal plate of the maxillary a little behind the interval between the third (*d*₃) and fourth (*d*₄) teeth in the present specimen. The vacuity behind the maxillo-palatal plates, which was filled up by the palatines, is of a nearly equilateral triangular form.

The premaxillaries (figs. 1, 3 and 4, *n*) are short in proportion to their height, are of a subquadrate form, with the part containing the mid-incisor, *i* 1, bent rather abruptly inwards to join its fellow below the nostril, the face being terminated by an obtuse broad muzzle. The right premaxillary is most entire; it is a little dislocated outwards, and has been separated about 4 lines from its fellow, and to a less degree from the maxillary of its own side. The upper border, which joined the nasal (*n*), is 9 lines in extent; that which joined the maxillary (*m*) is 1 inch 4 lines; that which joined its fellow (*n*) is 10 lines. The palatal plate (fig. 3, *n*) is obliquely grooved by the prepalatal or incisive foramina, the thin inner parietes of which have been broken away.

The portion of the nasal bones preserved (figs. 1 and 2, *n*) show them to have been long and rather broad, meeting above and bent, so as to form an arch: they formed, with the premaxillaries, the external bony aperture of the nostrils, the maxillaries being excluded.

The lower jaw (mandible, Plate XVI. fig. 6) is remarkable for the great proportion which the ascending ramus bears to the horizontal one. The part of the condyle preserved shows it to have been transversely extended and rather convex; the coronoid process rose above its level, and is separated from it by a deep emargination. The entry to the dental canal is 2 inches 2 lines below the condyle. From the condyle to the lower border of the ramus beneath the last molar tooth in place measures 4 inches; from the same part of the lower border to the socket of the outer incisor is 2 inches 9 lines, the depth of the ramus at the same part is 1 inch 4 lines. There is a faint linear impression along the part of the lower border which is preserved. The incisive part of the mandible slightly expands; the outer surface of the horizontal ramus is, lengthwise, convex at its posterior two-thirds, and concave in the rest.

The teeth preserved in the present specimen of *Nesodon ovinus* are,—three incisors, a canine, and five molars on the right side, upper jaw; the canine and five molars on the left side, upper jaw; and the outer incisor, the canine and five molars in the right ramus of the lower jaw.

The upper incisors, unequivocally defined by their position in the premaxillary bone,

diminish from the first (innermost) to the third, and vary more in form than size. The first (figs. 1 and 4, *i*₁) is curved, with the convexity foremost; the fang is slender and contracts as it penetrates the bone; the crown expands and presents the true trenchant incisive form, is flattened transversely at the fore-part, and slightly convex at the back, with the edge worn flat, but with a slightly raised thin enamel ridge. The second incisor (ib. *i*₂) does not quite touch the first; its crown is curved, with the most prominent convexity at its fore-part, but it is narrower than in the first, and is three-sided, the outer and hinder facets being broader than the front one. The enamel is not continued upon the hinder facet: the contracted fang shows it to be, like the first incisor, a tooth of restricted growth. It is inserted at the angle of the premaxillary, where the front wall bends to form the side wall, and is external and a little posterior to the first incisor. The third incisor (ib. *i*₃) is double its own breadth's distance from the second, and is implanted behind it, close to the suture with the maxillary; the crown is short, subtriangular, and so much smaller than the others, as almost to be termed rudimental; it is, however, worn obliquely, like the crown of the second incisor.

The canine (figs. 1, *d* *c* and 3, *c*) is intermediate in size between the third and second incisors; it is implanted near the suture: the summit of its crown, which was conical and subcompressed, is worn away; its outer side presents a middle convexity bounded by a narrow longitudinal channel near each margin.

The first grinder (ib. *d*₁) is about the size of the second incisor, and is $1\frac{1}{2}$ line distant from the canine; its crown is thicker as well as broader and longer than that of the canine; the external convexity is produced into a longitudinal ridge near the fore-part of the crown, and its summit forms the most prominent part of the grinding surface, which surface slopes away both forwards and backwards from it, the hinder, worn facet being much the largest. The base of the crown contracts and divides into two short nearly parallel fangs. The entire length of the tooth is 7 lines.

The second grinder (ib. *d*₂), of nearly twice the size of the first, close behind which it is placed, presents two longitudinal ridges dividing three grooves on its outer surface, and a triradiate termination of an inflected fold of enamel at the middle of the grinding surface. The length (vertical diameter) of the crown is 5 lines, and it is implanted by short diverging fangs, two of which are external, and are exposed in Plate XVI. fig. 5, *d*₂; the total length of this tooth is $7\frac{1}{2}$ lines.

The third grinder (*d*₃, figs. 1, 3, 5) presents somewhat more than a proportional increase over the second, the hind fourth of which it overlaps, its breadth being $6\frac{1}{2}$ lines. The anterior of the two longitudinal eminences on the outer side of the crown is more prominent and angular than the second convexity which has begun to expand and subside, and the anterior overlapping border of the crown is compressed and produced like a ridge. A fold of enamel penetrates the middle of the inner side of the crown, dips into its substance, contracts as it proceeds outwards and forwards, and bifurcates; behind this there is a small pit or island of enamel:

the postero-external angle of the crown is slightly produced; the increase in the size of the crown is chiefly in its antero-posterior extent and length, which latter is $7\frac{1}{2}$ lines before it divides into fangs, two of which are external, short and divergent. The total length of this tooth is 10 lines.

The fourth grinder (ib. *d.*), presents an increase of size in the same directions as the third was enlarged, but to a minor degree in antero-posterior extent than in length, in proportion to which the crown is narrow transversely at its grinding surface. The posterior external longitudinal eminence has much subsided, and has lost all the character of a ridge; the anterior external longitudinal ridge continues to be angular and well-marked; it is near the similarly produced ridge-like anterior angle of the crown, which projects outwards, in advance of the hind part of the third grinder. The bifurcating internal enamel-fold penetrates the anterior half of the grinding surface; a second wide simple fold, parallel with the first, answers, in this less worn tooth, to the enamel island in the third grinder; the postero-external angle is more produced than in that tooth. The length of the crown is 1 inch, following its curvature, which is now well-marked, and one-half of the crown penetrates the substance of the maxillary, contracting before it divides into fangs, two of which are external, short and divergent; the entire length of the tooth is 1 inch 5 lines.

The crown of the fifth molar (ib. *m.*) had but recently begun to protrude from its socket, and is only abraded at the anterior half of the grinding surface. The branches of the forked enamel-fold appear as two distinct folds, and there is a slight depression behind the beginning of the main fold. In the hinder lobe of the tooth there is a wide fold on the inner side, and a depression at its end, which would give rise to an island of enamel at a certain stage of abrasion of the crown. The thickness of the exposed and worn surface of this grinder does not exceed that of the preceding tooth; the breadth or antero-posterior extent is a little greater; but the length of the crown, which is imbedded in the jaw-bone, is considerably greater; it measures, following the outer convexity, 1 inch 8 lines, increasing in thickness, and slightly diminishing in breadth, and terminating in a wide pulp-cavity, without trace of fangs. The ridge-like anterior angle is produced external to, and in advance of, the fourth grinder, in the same imbricated manner as the fourth overlaps the third grinder, and this the second.

In the lower jaw the only incisor remaining (Plate XVI. fig. 6, *i.*) is that answering to the third in the upper jaw; but it is double the size of that tooth; the crown has the usual trechant inciseive form convex externally, slightly concave on the opposite surface, with a long, straight fang, contracting as it sinks in the socket.

The canine (ib. *c.*) is somewhat less, with the crown similarly compressed, but with its worn margin more angular than in *i.*

The first molar (ib. *d.*) much resembles the canine, but is somewhat larger, and the longitudinal convexity and depressions of its inner surface are more marked; its root is single and contracted like that of the canine.

The second molar (ib. *d.*) has been markedly larger than the first, but is too mutilated for description; it was implanted by two fangs.

The third molar (ib. *d.*), of still increasing size, and chiefly in antero-posterior extent, presents a long compressed crown, divided into two lobes, convex externally, by an oblique, unequal-sided longitudinal angular indentation: the inner side of the hinder and larger lobe is penetrated by two folds, extending obliquely outwards and forwards, fig. 8, *d.*

The fourth molar (ib. *d.*), with a slight increase of antero-posterior extent, has its anterior lobe indented internally.

The crown of the fifth molar (ib. *m.*) resembles that of the fourth in configuration and size, but is narrower, from having been less worn. The grinders are placed close together one behind another, in a straight line and a little overlapping.

All the teeth present a deep brown, in some parts almost black, colour, with a polished shining surface. The enamel is remarkably thin, and is closely blended with the dentine, which renders it very difficult to determine its extent upon the exterior of the crown: it appears to be continued much further upon the outer than the inner side of the tooth.

The incisors and canines being indicated by their position, there remained then to determine the nature of the five grinding teeth in place, on each side of both jaws. That this was not the entire number characterising the dental formula of the animal, was shown by the germ of a molar (Plate XVI. fig. 5, *m.*) in the fractured hinder protuberance of the maxillary, behind the last grinder in place, the crown of which (ib. *m.*) had very recently emerged from the socket.

To obtain further insight into that formula—so important in the elucidation of the true generic character and affinities of the *Nesodon*—I removed the inner parietes of the sockets of the molars in the right ramus of the lower jaw, and, excavating below the base of the coronoid process, exposed there the germ of a molar (ib. fig. 7, *m.*) answering to that forming the sixth (*m.*, fig. 5) in the upper jaw. The third (*d.*) and fourth (*d.*) grinders were implanted by two long tapering and diverging fangs; but the fifth grinder (*m.*) presented a marked difference; its crown being continued undivided, with a slight and gradual diminution in antero-posterior extent, and corresponding increase in transverse thickness, to a level, half an inch lower in the deepening ramus of the jaw, than that of the end of the fangs of the antecedent grinder, *d.*. The middle indentation on the inner side of the crown of the fifth grinder (*m.*) gradually terminates 9 lines below the summit, the rest of the inner side of the crown being almost flat: at its base a wide pulp-cavity is exposed (fig. 9, *m.*), into which the obtuse conical end of a deeply inflected enamel-fold projects from above, indicating great variety in the pattern of the grinding surface at different stages of abrasion of the crown.

From the difference between the fourth (*d.*) and fifth (*m.*) grinders, I conclude them to belong to different series of those teeth; and, from the analogy of the times

of their appearance in other *Herbivora*, I regard the fifth tooth in place as the first true molar (m_1 of my dental notation*), the succeeding germ to be the second true molar (m_2), and the preceding four grinders in place as being the deciduous grinders (d_1 to d_4). At the stage of growth here exemplified, with the crown of the first true molar (m_1) only beginning to emerge from its socket, the germs of the vertical successors (premolars) of the deciduous molars could scarcely be expected to be calcified; but the depth of the jaw beneath those molars, and the space provided by their narrow and diverging fangs, indicate a provision for lodging the matrices of successional teeth, and the small cavity in the substance of the jaw (fig. 7, p_1) beneath the fangs of the fourth molar (d_4) is probably the beginning of the formative nidus of its successor, the fourth premolar (p_4).

The small size of the incisors, in comparison with the premaxillary bones, and their scattered disposition therein, indicate them to belong to the deciduous series, to which, also, we must refer the diminutive canine.

The shape, size, disposition and complex structure of the molar teeth prove the present extinct species to be herbivorous, and the number and nature of the teeth exposed indicate it to be ungulate. No herbivorous or other Rodent has canines and six incisors in the upper jaw; and no Mammal of the Order *Bruta* (*Edentata*, Cuv.) has deciduous teeth properly so called, or molars divisible into two kinds. All the herbivorous Marsupials have a single pair of large procumbent incisors, and no canines, in the lower jaw.

In comparing the fragments of skull and the teeth above-described with the corresponding parts of existing *Ungulata*, we find the most numerous and important correspondences to be with those belonging to the order *Perissodactyla*†.

Both upper and lower true molars in the *Nesodon* have very long crowns, indicating the roots to be comparatively short, and formed late, as in the genus *Equus*. The close imbricated disposition of the upper molars and the pattern of the enamel-folds of the crown find their nearest parallel in the *Rhinoceros*.

Both the Tapir and the Horse resemble the *Nesodon* in the number and relative proportions of the incisors and canines. The Tapir, amongst existing *Ungulata*, offers the closest resemblance to the *Nesodon*, in the form and proportions of the zygomatic arch, of the widely communicating orbital and temporal fossæ, and of the articular eminence and cavity for the lower jaw. The premaxillaries join the nasals as in the Horse. The proportions and shape of the post-glenoid process in the *Nesodon* approach more nearly those in the Tapir or Horse, than those in any of the Artiodactyles‡ in which that process is well-developed, as *e. g.* the Peccari and Hippopotamus, in neither of which is the 'eminencia articularis' developed as in the *Nesodon*. The principal osteological character in which the *Nesodon* deviates from the perissodactyle, and seems to approach the artiodactyle, type, is the production of the osseous roof of

* Philosophical Transactions, Part II. 1850, p. 491.

† *Pachydermes à doigts impaires*, CUVIER.

‡ *Pachydermes à doigts paires et Ruminans*, CUVIER.

the mouth backwards as far as, and a little beyond, the last molar in place; but this part of the palate is formed for some extent externally by the maxillary bones (Plate XV. fig. 3, *n*), whilst in the Artiodactyles it is formed by the palatines, except where a very thin plate of the maxillary completes the socket of the last molar on its inner side.

Neither family nor subordinal correspondence resulting from the comparisons with existing *Ungulata*, they were next extended to the extinct forms, and especially to those that had previously been discovered in South America. But not to prolong the description with needless details, I may confine myself to the remark, that, in this inquiry, the nearest resemblance to the *Nesodon* was found to be offered by that hitherto anomalous genus for which I have proposed the name of *Toxodon**

In the *Toxodon platensis* the bony palate is continued, as in existing Artiodactyles, beyond the molar series; but the proportion of this back part which is contributed by the maxillaries resembles that in the *Nesodon*, and is greater than, and different in form from, that in the Hog-tribe, the Hippopotamus and the Ruminants. The palate is arched in the *Nesodon*, and also, but with a deeper concavity, in the *Toxodon*: the post-palatal canals in both have the same advanced position of their oblique outlets upon the maxillary; in both Perisso- and Artio-dactyles those outlets are close to the maxillo-palatal suture. In the form of the 'cavitas et eminentia articularis,' and of the post-glenoid process, the *Toxodon* more resembles the *Nesodon* than does the Tapir or the Horse: the convex and protuberant front and lower part of the process and the excavation of its back part are repeated in the *Toxodon*. The strong, deep, sigmoidally bent zygoma, and the vertically extended orbit communicating widely behind with the temporal fossa, are characters common to the *Nesodon* and *Toxodon*; but the orbit is relatively wider, the malar bone is narrower, and its post-orbital process less prominent in the smaller extinct South American Herbivore. In the concavity of the outer surface of the deep facial part of the maxillary, and the anterior expansion of the premaxillaries, the *Toxodon* resembles the *Nesodon*; but the most important evidence of their mutual affinity is afforded by the dental system.

The molar series has a similar arrangement in the *Toxodon*, describing a line gently convex externally, the teeth gradually decreasing as they advance, and having the same close-set, imbricated disposition. The length, curvature and deep implantation of the undivided crown of the upper molars of the *Nesodon*, as exemplified in the fifth molar of the specimen described, fig. 5, *m* 1, are important marks of agreement with the upper molars of the *Toxodon*, in which those characters are present in a greater degree. The same molars of the *Toxodon* also present an obvious similarity to those of the *Nesodon* in structure. The enamel is very thin: a deep oblique fold, penetrating from the inner side, divides the crown into two lobes; and a short fold indents the inner side of the hinder lobe. The anterior external angle of the tooth is produced like a ridge to form the overlapping part in the imbricated molar series:

* Zoology of the Voyage of the Beagle, Fossil Mammalia, p. 16, Plates I. to V. 4to, 1840.

there are, also, two longitudinal risings along the outer side of the tooth. The chief differences presented by the *Toxodon* are seen in the simplicity of the enamel-folds, the low and equal development of the two external longitudinal ribs or risings, the relatively narrower grinding surface, and the more evident interruption of the external coat of enamel at the anterior, posterior, and internal parts of the tooth. Moreover, there is no trace of canines or of outer incisors (*i.*) in the adult *Toxodon*, and the anterior upper incisors are proportionally very long and large; but the difference might be less in the permanent upper incisors of the *Nesodon*.

The correspondence between the teeth of the lower jaw of the *Toxodon* and *Nesodon* is of the same instructive character. No quadruped, recent or extinct, presents so close a resemblance to the lower molars of the *Toxodon*, in regard to their narrowness, and the length and deep implantation of the straight undivided crown, as does the *Nesodon*, especially in the first true molar, exposed in the section shown in fig. 7, *m.* 1. The (most probably) deciduous incisors of the same specimen deviate, however, considerably from the peculiar disposition, size, shape and curvature of the six lower incisors in the adult *Toxodon platensis*, and this difference becomes of more importance as illustrative of the generic distinction of the two animals, from the proportions and arrangement of the permanent incisors and canines in the lower jaw of the adult *Nesodon*, first noticed in the 'Reports of the British Association' for 1846 (Transactions of the Sections, p. 66), where the affinity of the *Nesodon* to the *Toxodon* is indicated, and the interval between the latter genus and *Macrauchenia* is stated to be "partly filled by the newly-discovered *Nesodon*."

We may, now, therefore, regard the following as the generic characters of the *Nesodon* :—

Dental formula, — $i \frac{2-3}{1-2}$, $c \frac{1-1}{1-1}$, $p \frac{4-4}{1-2}$, $m \frac{2-2}{1-2}$ = 44. The teeth subequal, and in an unbroken series. *Incisors* trenchant, with long, slightly curved crowns, but not disproportionately large, and of limited growth, being implanted by a tapering fang: *Canines* smaller, and not exceeding in length the contiguous premolars: *Molars*, above, with long, curved, transversely compressed crowns, which contract, as they penetrate the bone, and ultimately develope fangs: the outer side of the crown ridged, the inner side penetrated by two more or less complex folds of enamel, leaving insular patches on the worn crown: the enamel is deposited in a thin layer. The lower molars with long and straight, transversely compressed crowns, divided by an external longitudinal indent into two unequal lobes, both penetrated at the inner side by a fold of enamel, which is complex in the hinder lobe.

The bony palate entire and extending back beyond the molars, the maxillaries and palatines forming the back part in equal proportions. A distinct articular cavity and eminence for the lower jaw, the eminence long and concave transversely, short and convex longitudinally; a protuberant post-glenoid process; a strong and deep zygoma; the orbit and temporal fossa widely intercommunicating; premaxillaries joining the nasals.

Of the genus offering the above osteological and dental characters, I have already received evidence of four species.

The first is that about the size of the Llama, called *Nesodon imbricatus**, founded on the fore-part of the lower jaw and on two molars of the upper jaw. The second species, of the size of the Zebra, called *Nesodon Sulivani* in honour of its discoverer, is indicated by detached molar teeth of the lower jaw. The species to which the portions of skull above described belonged, seems, from the size of the first and second true molar tooth, to have been about the size of the *Vicugna*, or of a large sheep, and I propose to call it *Nesodon ovinus*. A fourth species, as large as a Rhinoceros, is indicated by an upper molar tooth; and I propose to call it *Nesodon magnus*.

That the specimens on which the species *Nesodon ovinus* is founded, did not belong to a young individual of the *Nesodon imbricatus*, is shown by the difference in the extent occupied by the first four (deciduous?) grinders in the lower jaw, Plate XVI. figs. 6, 7, 8, *d 1-4*; and in that occupied by the first four grinders (premolars) in the lower jaw of the *Nesodon imbricatus*, Plate XVII. fig. 11, *p 1-4*; and by the great difference in the size of the teeth, which, on the hypothesis of the two alleged species being one, must hold the relation of temporary and successional teeth. For, assuming, as is most probable, that the first four grinders in the lower jaw of the *Nesodon ovinus* are milk-teeth; yet these are displaced and succeeded vertically by the premolars, which are in most Herbivores smaller and less complex, and in no Mammals larger and more complex, than the teeth they so displace; such premolars never occupying a greater longitudinal extent, although they sometimes, when fewer in number than the deciduous grinders, occupy a less extent, in the jaw of the adult animal. We may therefore safely infer that the four teeth occupying the extent of 3 inches in the lower jaw of the *Nesodon imbricatus*, *p 1-4*, fig. 11, could not have risen into the place of the four molars, which only occupy the space of 1 inch 9 lines in that of the *Nesodon ovinus*, *d 1-4*, fig. 6.

If, next, we compare the crown of the first true molar in the upper jaw of the *Nesodon ovinus* (fig. 3, *m 1*) with either of those (fig. 10) referred in my former Memoir† to the *Nesodon imbricatus*, we shall obtain equally conclusive evidence that they have not belonged to the same species.

Upper Molar Teeth of NESODON IMBRICATUS.

The antero-posterior extent of the crown of *m 1* in the *Nesodon ovinus* is 9 lines; in the *Nesodon imbricatus* it is 15 lines; the greatest transverse diameter of *m 1* in *N. ovinus* is 5 lines; in *N. imbricatus* it is 9 lines. The generic conformity of the shape of the crown and the disposition of the enamel is very close. Both the upper molars (Plate XVII. fig. 10) referred to the *Nesodon imbricatus* have been much worn, the anterior one, of course, the most. In this tooth the posterior division of the forked

* Reports of the British Association, 1846, p. 66.

† Loc. cit. p. 67.

fold, *a*, is obliterated except at its end, which forms the enamel-island *b*, fig. 10, and the deep termination of the second fold forms the second island, *c*, which is separated from the rest of the fold, which also forms an island, *d*.

In the next molar, the deep beginning of the second fold, *d*, still communicates with the enamel covering the inner side of the molar: the two branches of the forked fold, *a*, retain the continuity with the main stem, and the only island is that (*c*) formed by the sunken end of the second fold. On the implanted side of the crown of the first molar the obtuse blind ends of both folds project into the bottom of the pulp-cavity of the crown, from which the beginnings of short roots are seen to diverge. The bases of the two folds are seen to be more distinct in the wider pulp-cavity of the second and less worn tooth. The crowns of both teeth have had the same curved form as in the *Nesodon ovinus*; but the greater part has been worn away.

Part of the Lower Jaw and some Teeth of the NESODON IMBRICATUS.

The fossil which originally indicated the genus, and was referred to the species *imbricatus**, consists of the symphysis and part of the right ramus of the lower jaw (Plate XVII. figs. 11, 12, 13, 14). The symphysis is obliterated by complete confluence of the rami at that part; it is rounded anteriorly and slopes downwards and backwards at an angle of 120° with the lower border of the rami; these are slightly concave along the outer walls of the sockets of the anterior premolars, as if the rami had been gently pressed together at that part, in advance of which the symphysis slightly expands before forming the convex termination supporting the incisors.

These teeth (*i* 1, 2, 3, figs. 11, 12, 14) incline forwards at the same angle as the symphysis, and increase in size from the first to the third; the summits of the crowns have been worn to an almost flat elongated oval or crescentic surface, from $1\frac{1}{2}$ to 2 lines in the smaller diameter. Those of the first pair (*i* 1) are in contact, the contracting bases slightly diverging to their insertion, which is by a long and slender fang; the length of the enamelled crown is 8 lines, the breadth at the worn summit $4\frac{1}{2}$ lines; this is thickest at the inner border, when the thickness so increases towards the fang as to give a three-sided figure to the transverse section of the base of the crown: the length of the fang is 1 inch, fig. 12.

The second incisor (*i* 2) is situated partly behind and to the outer side of the first, so that the mesial angle of the crown is overlapped anteriorly by the outer angle of that of the first incisor. The crown of the second has its anterior surface almost equally divided into two facets, which meet at an angle which marks the extent of the overlapped part of the crown. The posterior surface of the crown is divided unequally, the intervening angle or longitudinal eminence being near the inner rounded border of the crown. This is 9 lines in length, and nearly 6 lines in breadth at the worn surface. The fang contracts as it descends in the socket, and is 1 inch 1 line in length, fig. 12.

* Loc. cit. p. 66.

The third incisor (*i*₃) has the anterior surface turned more outwards, but similarly subequally divided into two facets, the mesial one lying directly behind and upon the second incisor; the posterior surface is more flattened; the inner border is sharper and more produced; the outer angle of the crown is rounded off. The length of the crown is 10 lines, its greatest breadth, which is below the worn end, is $7\frac{1}{2}$ lines; the fang is somewhat shorter and thicker, but contracts, like the others, to its end; its length is 11 lines, fig. 12.

The canine (*c*) rises and slopes forwards close behind the third incisor; it is of smaller size, with the fore-part of the crown more convex, and unequally divided by a longitudinal eminence nearer its outer border; the posterior surface is concave; the summit is thin, rounded, with the enamel not worn off, although it is on the same level with those of the incisors. The crown is 9 lines long and 6 lines broad, half-way down. The enamel is continued much further down the fore than the back part of the crown in the incisors and canines, and the length of the crown has been taken from the fore-part. The fang of the canine (*c*, fig. 12) is 7 lines in length.

The first premolar (*p*₁) rises behind and to the inner side of the canine; the two surfaces of the crown are directed outwards and inwards; the outer surface has its anterior half convex, its posterior half concave; the convex part rests on the concave surface of the canine, and the concave part receives the lower part of the anterior convexity of the second premolar. The working border rises to an obtuse point formed by the summit of the longitudinal prominence dividing the two parts of the outer surface. The inner surface is slightly concave anteriorly. The height or length of the crown is 9 lines, the breadth of the crown is 8 lines. It is implanted by a single, short and thick, moderately tapering, and obtusely ended fang, 6 lines in length (*p*₁, fig. 12).

The second premolar (*p*₂) has the crown divided into two equal lobes by a wide and deep longitudinal angular indentation on its outer surface. The anterior lobe is the most prominent one, and has three sides, the antero-external surface being transversely convex; the postero-external one flat, and the internal surface concave. The inner concavity of the hinder lobe sinks at its lower end into the substance of that lobe. The whole crown is convex vertically on the outside, concave on the inside, being slightly bent inwards in the direction of its length; this measures 10 lines, the breadth is 8 lines: the crown gradually contracts to the fangs (*p*₂, fig. 12), which are two in number, and diverge as they penetrate the sockets; their transverse breadth is considerable; their length 6 lines. The anterior one passes external to the fang of the first premolar, which is wedged, as it were, into the internal interspace between the canine and the second premolar.

The third premolar (*p*₃, figs. 12 and 14) presents the general complex form and structure characteristic of the true molars. The external entering angle marks off the anterior third of the crown as the anterior lobe, which is convex transversely,

and more prominent than the larger and less convex posterior lobe; it is slightly indented on the inner side. The posterior lobe shows the two islands of enamel *a* and *b*, fig. 14, Plate XVII. on its grinding surface and the terminal fossa (*c*) of the internal longitudinal fold almost reduced to the state of an island. The length of the crown is 9 lines, the breadth (now the antero-posterior extent) is 11 lines, the thickness of the worn surface of the anterior lobe 4 lines.

The crown of the fourth premolar (*p*₄, figs. 11 and 14) is 1 inch (12 lines) in breadth, but sinks much deeper in the substance of the jaw before dividing. The narrow curved enamel-fold, answering to the island *a* in *p*₄, is seen in the present less-worn tooth to be a fold marking off the hind boundary of the anterior lobe on the inner side of the crown: the circular island (*b*) is repeated; the inner fold (*c*) of the hind lobe is wider. The grinding surface of the fourth premolar does not exceed that of the third in breadth.

Only the anterior lobe of the first true molar (*m*₁) is preserved; it has been worn down two lines below the level of the last premolar, and the internal depression is wider and deeper than in the premolars. Its length is 1 inch 10 lines, of which more than 1 inch is implanted in the jaw, and it terminates below in a widely open pulp-cavity, fig. 13.

The last three premolars and first true molar are in close apposition, one behind the other in the same line; the anterior surface of one being pressed so close against the back part of the next as to have caused the disappearance of the enamel, if it had been originally laid upon the anterior surface of the crown so worn by pressure.

The part of the jaw here preserved shows it to have been deep and narrow; the hinder half probably resembled in its height that in the *Nesodon ovinus*. Two small foramina on the same horizontal line open upon the outer surface, midway between the upper and lower borders; one of them 1 inch 9 lines behind the anterior end of the symphysis, the other 2 inches 3 lines behind the same part. The antero-posterior extent of the symphysis seems to have been not more than 2 inches.

The breadth of the jaw at the alveoli of the first premolars is 1 inch 8 lines; it becomes wider below, as well as in front and behind those teeth.

The lower molars of the *Toxodon*, in their great length of the undivided crown, their breadth, their thinness, the narrow and prominent anterior lobe, and the broader but thinner and internally penetrated posterior lobe, manifest the same general type of structure, as those above-described, in the *Nesodon*; the generic distinction is seen in the less complex disposition of the enamel.

When the lower jaw and teeth of the *Nesodon imbricatus* are compared with those of the *Nesodon ovinus*, differences present themselves, which can only be referred, in the present state of zoological principles or philosophy, to a distinction of species.

The crown of the outer incisor in *Nesodon ovinus* (Plate XVI. figs. 6 and 8, *i*₂) is less than half the size of that in *Nesodon imbricatus*, so likewise is the crown of the canine and first grinder; and, supposing these teeth to be the deciduous ones, they

are proportionally much smaller than the same milk-teeth are in comparison with their permanent successors in the Horse or Ruminant. In no existing Herbivore is the crown of the third or fourth premolar longer than that of the third or fourth corresponding milk-teeth; but the antero-posterior extent of the crown of the fourth premolar in *Nesodon imbricatus* equals that of both third and fourth (milk?) molars in the *Nesodon ovinus*.

The difference of the size of the fifth molar (m_1) in *Nesodon ovinus*, compared with the portion of the first true molar (m_1) in *Nesodon imbricatus*, is decisive as to the superior size of that species to the adult of the *Nesodon ovinus*, on the hypothesis that the teeth anterior to m_1 are deciduous in the specimen on which I have founded the latter species.

The summit of the thin but broad crown of m_1 , fig. 7, has but just begun to be abraded in the *Nesodon ovinus*; but the entire extent of the inflected fold of enamel is displayed in the imbedded part of the tooth, which has been exposed at m_1 , figs. 7 and 9, the vertical extent, from the summit of the tooth, being 1 inch 4 lines.

The vertical extent of the part of the first molar (fig. 13, m_1) preserved in the lower jaw of the *Nesodon imbricatus* is 1 inch 10 lines, and the crown continues to expand to the widely open lower termination, which measures 6 lines in thickness, the same diameter of the bottom of the crown of the first molar (m_1) in the *Nesodon ovinus* being 2 lines.

Here, then, we have two species of the extinct genus restored; and the fossils upon which the *Nesodon imbricatus* is founded supply the characters of the permanent teeth which are so singularly clustered in overlapping arrangement, at the fore-part of the lower jaw, whilst the characters of the first or deciduous dentition of the same part of the jaw are illustrated in the more complete fossils that give the characters of the *Nesodon ovinus*.

Lower Molar Teeth of NESODON SULIVANI.

I next proceed to the description of the remains of the third species of the present remarkable genus.

Nesodon Sulivani.—This species, which appears to have been about the size of a Zebra, was founded, in my original Memoir*, on some detached teeth, of which I now subjoin more detailed descriptions, with figures.

Plate XVIII. fig. 15, represents a lower grinder, with the crown worn down, and the root broken away, an equal extent of both parts of the tooth being preserved. The root has been fractured at the part where its two divisions had not begun to separate; but where their proportions are marked out by a median external and internal longitudinal indentation, connected by a line traversing the consolidated substance of the fractured base, and indicating it to consist of two connate fangs. By the equality of the two lobes of the crown, indicated by the external longitudinal

* Loc. cit. p. 67.

notch, *n*, this tooth corresponds best with the second premolar (*p* 2) in the lower jaw of the *Nesodon imbricatus*, fig. 11; and, the anterior lobe being indicated by its greater prominence, it is shown to have come from the right ramus of the jaw. The crown has been worn below the part which bears the internal depressions in the *N. imbricatus*; and a smooth field of dentine is exposed, with the indented plate of enamel on the outer side, and a smooth, slightly convex plate of enamel on the inner side, the two plates being interrupted on the anterior and the posterior sides of the crown, due apparently to the unequal degrees in which the enamel was extended towards the fang along different parts of the crown. The outer coat of enamel is thicker than the inner one; and the former best shows the delicate parallel close-set transverse striae, indicative of the successive formation of that substance; it is also impressed by a transverse row of minute close-set punctations. The fang is enclosed by a thin layer of cement, which is continued upon the parts of the crown undefended by enamel. The hinder lobe is worn lower than the other, as it is also in the *Nesodon imbricatus*. It differs from the tooth of corresponding form in that species, not only in its greater size, as indicated by the dimensions of breadth and thickness, but also by the thicker coat of enamel and the greater length of the undivided root. There is, also, a worn, smoothly excavated surface on the fore-part of the crown, which indicates close contact with an anterior premolar of a different shape from the *p* 1 in the *Nesodon imbricatus*.

The length of the enamelled crown here remaining is 10 lines, its breadth 9 lines, its thickness 6 lines.

A portion of the corresponding tooth of the left side of the same jaw is preserved, in which the bottom of the enamel-fold is still unobliterated, answering to the island, *a*, *p* 3, fig. 14, in the second premolar of the *Nesodon imbricatus*. The length of this fragment is 1 inch 9 lines; its anterior surface shows the same abrasion from pressure against the tooth in advance, and the fractured surface exposes a contracted pulp-cavity in the crown, which becomes obliterated in the fang.

The tooth, Plate XVII. fig. 16, apparently the premolar (*p* 3), succeeding the one last described, is also worn down so near the bottom of the crown, that the inequality of the two lobes appears much less than it would be if the same proportion of the crown had been preserved as in *p* 3 of the *Nesodon imbricatus*; it is probably, therefore, notwithstanding the actual difference in the proportions of the two lobes, the tallying tooth. Of the inflected fold of enamel only the bottom of one remains, forming an island (*a*) in the substance of the grinding surface of the crown, opposite the angle of the fold, *n*, indenting the outer surface of the crown.

The difference in thickness of the outer and inner plates of enamel, which are disconnected at the fore and back parts of the grinding surface, is well-marked, that of the inner plate being less than that of the insular fold, *a*. The inner plate terminates below by a narrow process continued upon the base of each division of the fang. The base of the crown begins to divide above the lower boundary of the outer plate

of enamel, into two almost consolidated fangs, the ends of which are broken off, showing in each the very contracted remnant of the pulp-canal. The enamel is continued 9 lines below the termination of the external groove, is continued a short way down each division of the root, and is characterized by four transverse rows of close-set small circular pits. The length of the enamelled crown is 1 inch 5 lines, its breadth 11 lines, its thickness 8 lines. The tooth above described is from the left side of the jaw; its fellow from the right side is preserved, but in a more mutilated condition.

A tooth, Plate XVII. fig. 17, which, in the unequal size of the two lobes of the crown, tallies with the third and fourth premolars of the *Nesodon imbricatus*, and, by its superior size to fig. 16, answers to the fourth premolar (*p* 4), has had the complexities of the grinding surface reduced by extensive attrition to the bottom of the deepest of the internal folds, *a*. It consequently resembles the foregoing tooth (fig. 16), except in its superior size, and in the greater extent of the hinder lobe, which is less convex externally. The outer plate of enamel, below the longitudinal indentation, shows the same coarse and punctate transverse markings. The more regular and delicate striæ of growth are beautifully and clearly shown. The posterior of the two roots, which is the largest, has a narrow central pulp-cavity. The external coat of cement is continued a little way upon the enamelled parts of the crown, and very plainly coats the anterior and posterior tracts where the enamel is interrupted. The length of the remainder of the enamelled crown of this tooth is 2 inches 2 lines, its breadth 1 inch 1 line, its thickness 7 lines.

A portion of tooth (Plate XVII. fig. 18), including the base of the outer lobe of the crown, corresponds in colour, structure and markings with the same part in the foregoing tooth, but is somewhat larger; and has had a much larger crown; it is most probably a part of the succeeding grinder in the same jaw, viz. the first true molar, *m* 1. An extent of 1 inch and 6 lines of the outer enamelled part of the outer lobe is preserved, yet the anterior margin of the enamel plate is straight, vertical, and the cement covering the anterior surface of the same shows no trace of abrasion from pressure against the adjoining tooth, as in *p* 4, at the same distance above the lower border of the enamel covering the external surface of the anterior lobe. The striæ of growth are as conspicuous upon the enamel of the present tooth (*m* 1) as in that of *p* 4; and there are two transverse rows of punctations. The lower pointed termination of the internal plate of enamel is preserved at the back part of the fragment; it is 1 inch 4 lines higher than the termination of the enamel on the fore-part of the same tooth.

The tooth, fig. 18, as the reduced size of the enamel-island *a* demonstrates, has had more of its crown worn away, has been longer in use than the one, fig. 17, which, on the conclusion that it is a premolar (*p* 4) of the same jaw, must have stood in advance of fig. 18; but, though more worn, what remains of the crown of fig. 18 is longer. The first condition—greater degree of abrasion—accords with the deter-

mination of this tooth as the first true molar, m_1 ; and, if the tallying tooth in the lower jaw of *Nesodon ovinus* be referred to, Plate XVI. fig. 7, m_1 , it will be seen that that tooth would most probably have a longer crown than the successor of d_4 , and would most certainly show a more worn-down summit when the fourth premolar had come into use in advance of it. These evidences of the nature or homology of the fragment of tooth, fig. 18, strengthen my surmise that the teeth (figs. 15-20, Plate XVIII.) are all from the same under jaw.

Plate XVII. fig. 19, is a part of the inner enamelled portion of the crown of a molar 3 inches and a half long, including part of a widely open pulp-cavity at the base, but with an unknown quantity of the worn or exposed end of the crown broken away; it indicates, therefore, a straight, rootless, molar of considerable length; but the character of the enamel accords so closely with that of the better-defined broken teeth above-described, as to indicate it to have belonged to the same genus, species, and, probably, individual, as the other teeth with which it was associated.

The enamel is thinner at its upper broken end than that on the outer side of the above-described teeth of the lower jaw; and it becomes still thinner as it approaches its lower termination. It forms a nearly flat, long plate, terminating by a free, thinned-off border on each side along its lower half, where its breadth is from 8 to 10 lines. The upper half-inch of enamel shows the transverse wavy striæ of growth; the next inch is roughened by the punctate impressions and coarser transverse lines; then the regular striæ of growth are continued for two-thirds of an inch; and again, the punctate character appears for an extent of 3 lines, the alternation of striated and punctated tracts continuing to the irregular rugged termination of the enamel plate. A thin layer of cement covers the outer surface of the lower part of this fragment, which is not plated by the enamel.

Plate XVII. fig. 20, is a similar but smaller portion of a somewhat larger, long and straight tooth, nearly 2 inches in length, and with the enamel-plate 11 lines in breadth, terminating on each side in a thin free border. Both upper and lower ends of this portion have been broken away, the latter above the pulp-cavity; and the enamel and cement here enclose a solid mass of dentine, of a three-sided shape, one of the sides being much narrower than the other two, indicating a long straight tooth, thus in proportion to its breadth. The outer surface of the enamel shows the same alternation of striated and punctated transverse markings as in the preceding fragment.

As the teeth in the portion of jaw of the *Nesodon imbricatus* (Plate XVII. figs. 11-14) are all of the permanent series, the differences in respect of size which they present in comparison with their homologues above-described, are decisive as to their belonging to a smaller variety, if not, as is more probable, to a smaller species. That the degree of distinction is specific, is further indicated by the punctate markings of the enamel, and the more strongly marked striæ of growth; also, by the greater relative thickness of the teeth of the *Nesodon Sulivani*, and by their relatively as well as absolutely

thicker enamel. The outer surface of the anterior lobe of p_2 , and of both lobes of p_3 , is less convex in *Nesodon Sulivani* than in *Nesodon imbricatus*.

Upper Molar Tooth of NESODON MAGNUS.

The superiority of size, indicated by the portion of tooth in Plate XVII. figs. 21, 22 and 23, of the animal to which it belonged, over the *Nesodon Sulivani*, is too considerable to be interpreted as a mere variety in a wild animal. That the dental fragment in question belongs to a species of the present genus, is to be inferred by the close resemblance of the enamelled exterior surface of the tooth to the same part in the upper molars of the known species of *Nesodon*.

This remarkable fragment, which might pardonably have been mistaken for part of the tooth of a Rhinoceros, is the outer side of the crown of an upper molar, worn low down, showing the natural termination of the enamel upon the base of the fangs which, with the inner part of the crown itself, have been broken away.

The remaining part of the crown presents its grinding surface worn obliquely to the enamel, which forms almost a trenchant edge; the produced and ridge-like anterior angle (fig. 23, *a*) is preserved; the longitudinal obtuse ridge of enamel (fig. 21, *b*) on the outside of the crown, near that border, and the gently convex surface of the broad part of the crown behind the ridge, equally repeat the characters of the outer surface of the upper grinders of *Nesodon*, as exemplified in the smaller species (fig. 5, *d*). A narrow and low ridge of enamel is continued from the base of the longitudinal rising, *b*, along that of the rest of the crown, 3 or 4 lines from the radical border of enamel, and then curves upwards along the hinder border of the crown to the posterior angle of the outer part of the grinding surface, *c*.

The crowns of the upper molars of the *Nesodon ovinus* do not show this marginal ridge.

The breadth of the remains of the crown of the upper molar of the *Nesodon magnus* at the grinding surface is 2 inches 8 lines; the length of the enamelled part is 2 inches 4 lines. The resemblance of the fragment in its shape, and in the disposition of the outer plate of enamel, to the similarly sized upper molar of the *Rhinoceros* is very close; but the characteristic thinness of the crown, as shown by the bend of the enamel at the fore-part of the fragment, fig. 23, the thinness of the enamel, and the uniform oblique abrasion of the outer part of the grinding surface, are all marks of closer resemblance to the upper molars of the genus *Nesodon*.

The surface of the enamel in the present tooth of *Nesodon magnus* is polished, but is minutely wrinkled, especially towards its basal termination; it shows no striæ of growth.

The remarkable portion of molar tooth here described and figured indicates a species of *Nesodon* of a size equal to that of the largest extinct species of *Rhinoceros*, which genus we may suppose the *Nesodon* to have represented in the American Continent during the pliocene and perhaps miocene periods.

Concluding Remarks.

The osteological characters defining the orders of hoofed quadrupeds, called *Proboscidea*, *Perissodactyla* and *Artiodactyla*, are associated with modifications of the soft parts of such importance, as not only to establish the accuracy of the principles of that ternary division of the great Natural group of *Ungulata**, but to indicate that the known modifications of the skeleton of the extinct *Toxodons* and *Nesodons* of South America, in the degree in which they differ from the osteology of the already defined orders of *Ungulata*, must have been associated with concomitant modifications of other parts of their structure, which would justify, and indeed compel, the consistent classifier to place them in a distinct division of the *Ungulata*, of equal value, if not with the *Perissodactyla* and *Artiodactyla*, at least with the *Proboscidea*. Like the *Proboscidea*, this group, which I propose to call *Toxodontia*, is more nearly allied to the Perisso- than to the Artio-dactyle orders.

This is shown by the large and complex third and fourth premolars (p_3 and p_4), by their close similarity with the true molars, by the unsymmetrical oblique foldings and islands of the enamel, and by the great length of the crowns of the molars, to which the Horse alone offers any near approach amongst existing Ungulates. By the form and proportions of the eminentia articularis, of the glenoid cavity, and of the post-glenoid process,—and by those, also, of the lacrymal bone, of the zygomatic arch, and of the orbit,—the *Toxodontia* are most closely matched by the Tapir and Rhinoceros in the Perissodactyle order.

The dental and osteological characters detailed in the text, whilst they illustrate the closer mutual affinities between the Nesodons and Toxodons, establish their claim to be regarded as types of a distinct order of *Ungulata*; and they also tend to dissipate much of the obscurity supposed to involve the true nature of the genus *Toxodon*, and to reconcile the conflicting opinions as to its proper place in the Mammalian Class.

The fossils above described were discovered on the coast of Patagonia to the south of Port St. Julian, and my friend Mr. CHARLES DARWIN, F.R.S., has kindly communicated to me the following opinion as to the formation in which they were imbedded:—"These beds resemble mineralogically the upper ancient tertiary formation of Patagonia, but EHRENBURG found the included microscopical organisms wholly different from those of the ancient tertiary formation, being of freshwater and brackish origin (p. 117 of my Geological Observations on South America). Hence these beds are of unknown age, probably younger than the old tertiary and older than the superficial beds in which *Macrauchenia* was found."

* Memoir on the Anthracotheroid Animals, in 'Quarterly Journal of the Geological Society,' Nov. 1847, and 'Osteological Catalogue of the Museum of the Royal College of Surgeons,' 4to, p. 629.

DESCRIPTION OF THE PLATES.

PLATE XV.

Nesodon ovinus.

- Fig. 1. Side view of a portion of the skull.
Fig. 2. Upper view of the same.
Fig. 3. Under view of the same.
Fig. 4. Front view of the right premaxillary and incisor teeth.

PLATE XVI.

Nesodon ovinus.

- Fig. 5. Portion of the left upper maxillary bone, with the roots of the teeth exposed.
Fig. 6. Inside view of the right ramus of the lower jaw.
Fig. 7. Outline of the same, with the implanted parts of the teeth exposed.
Fig. 8. Grinding surface of the teeth.
Fig. 9. Under surface or border of the same ramus.

PLATE XVII.

Nesodon imbricatus.

- Fig. 10. Grinding surface of two molars, right side upper jaw.
Fig. 11. Part of the right ramus of the lower jaw.
Fig. 12. Part of the left ramus of the same jaw with the roots of the teeth exposed.
Fig. 13. Transverse section of the right ramus of the same jaw, showing the implanted part of the anterior lobe of the first true molar.
Fig. 14. The grinding surface of the teeth in fig. 11.

PLATE XVIII.

- Figs. 15-20. Portions of lower molar teeth of the *Nesodon Sulivani*.
Figs. 21-23. Portion of an upper molar tooth of the *Nesodon magnus*.

The figures, letters and symbols, are explained in the text.